

Psychological Review

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CONTENTS

The Paradox of Geneticism in Psychology: COLEMAN R. GRIFFITH 201

Psychology's Progress and the Armchair Taboo: D. B. KLEIN 226

A Theoretical Interpretation of Shifts in Level of Aspiration:
LEON FESTINGER 235

The Problem of Multiple Psychological Languages: HENRY WINTHROP. 251

Functional Autonomy of Motives as an Extinction Phenomenon:
DAVID C. MCCLELLAND 272

The Concept of Adjustment and the Problem of Norms:
ROBERT P. HINSHAW 284

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THE PSYCHOLOGICAL REVIEW

THE PARADOX OF GENETICISM IN PSYCHOLOGY

BY COLEMAN R. GRIFFITH

University of Illinois

I. INTRODUCTION

Men who work with the basic principles of the various sciences often face a difficult choice when new facts of notable import are first discovered. On the one hand, they can simply add the new data to the already existing structure of fact and principle, making such shifts in emphasis and minor amendments as seem to be required. This appears to be the usual practice. On the other hand, however, they can seek to determine what the new discoveries will mean for the reconstruction of the basic frames of reference of the discipline as a whole, and for the redefinition of its cardinal concepts. Some discoveries, therefore, are factual extensions of old principles to new decimal places. Others, by contrast, require new foundations for all decimals, and especially for the generalizations derived from every increase in precision. When, for example, new data about light, quanta, and the position of the observer entered the stream of physical thought, and when the properties of certain equations were limned against a non-Euclidean geometry, vigorous attempts were made simply to add what had been found to the formal structure of classical physics. It was soon discovered, however, that the new data required a substantial modification of the underlying forms of thought. This proved to be true both as regards the abiding concepts of matter and energy, and as regards the creatures who generate concepts. Not

even yet are the full ramifications of the necessary reconstruction of physics in view.¹

In a similar way, when the biological doctrine of evolution was first crystallized into an intelligible theory, the initial impulse was simply to add it to the existing concepts and principles of experimental psychology, in spite of the fact that practically all of the guiding themes in the interpretation of mind and action had been derived from frames of reference which were wholly static and absolute in systematic value. This attempt was made simply by clothing these themes with the notion of becoming, and adorning them with variation and natural selection. It was quickly discovered, however, that something much more profound was implied, both for psychology and for its philosophical presuppositions (32, 33). By inference, and sometimes by direct assertion, it was argued that the whole theoretical system of psychology must be altered, and yet, in all sober truth, the science of human nature, in its various systematic patterns, has not known what to do with the concept of evolution. It has not always known quite what to do even with the more customary notions of individual mental growth or development. In spite of obvious facts which support the argument that there are two dimensions of mind, the vertical and the horizontal (68), or the longitudinal and the cross-sectional, in spite of persuasive attempts to derive basic psychological functions from the longitudinal rather than the cross-sectional dimension (35), and in spite of a growing volume of data properly called genetic,² no one of the systems of psychology has been able to formulate a straightforward set of propositions about mental becoming. One of them, topology, has seemed to imply that the problem of geneticism, that is to say, the historical or biographical method, is irrelevant to a science of human nature (21, pp. 38-39; 63, Chap. I).

¹ This process of reconstruction in physics has been most clearly described by Einstein and Infeld (36). The search for the best methods of reconstructing the science of psychology is the common theme in an increasing number of papers in *THE PSYCHOLOGICAL REVIEW*.

² As, for example, comparative psychology (100), child psychology (73), anthropology (60) and various attempts at a systematic fusion of all three lines of evidence (72, 104).

The genetic point of view, then, occupies a paradoxical position in contemporary psychological thought. As a growing system of facts, genetic psychology appears to lie within the boundaries of the science as a whole, and even more strikingly, within the boundaries of many of its isms, but the notion of mental becoming does not exist as a part of the essential structure of any of them. As far as vehement assertions are concerned, developmentalism has induced an incredibly complex change in points of view toward human nature, and toward the social and sociocultural disciplines radiating from it, but many of the static absolutes of a non-genetic era remain quite untouched by it. For example, it is now a matter of historical record that Darwinism, through the concept of adjustment, has renewed interest in the quite common-sense notion of the activities of the mind (92). The functional revolt of Angell (2, 3) and Carr (26) was not against a steady current in psychology but against the static, logical and analytic bias of introspectionism. As an advance over, not to say modernization of, the cruder notions of mental activity, the functionalist accepted practically all of the analytic gains made by the structuralist; but it was held that the mental elements, and most of the other products of introspective description must, when taken adjustment-wise, render an adaptive service through genuine contributions to performance (25). The argument was that, if this were not true, there would be no reason why states of mind, or the neural conditions which subsidize them, should have survived, and be available for description during a specious 'now.'³ In addition to this relation to introspective psychology, the developmental point of view enormously hastened the creation of an animal psychology which slowly freed itself from the anecdotal method (99). This, to be sure, is a highly important fact in its own right, but it is an even more significant fact for the whole psychological enterprise. The reason is that it strengthened the position of a wide variety of behaviorisms and brought the nature of experimental method in psychology to explicit formulation (103). The early be-

³ As is illustrated in particular cases by Culler (30) and Hull (52).

haviorisms, of course, were off on the wrong track. Aside from describing the instruments of adjustment rather than adjustment itself, they hastened the shift from the logical practice of extrapolating downward from the minds of men toward the minds of infants and lowly animals to the equally logical practice of extrapolating upward from the lesser, that is to say, from the reflexive and instinctive stages of mentality toward the 'higher' mental life of man. There was, however, a net gain for, by a kind of dialectic, both practices were soon to be resolved in an operational behaviorism by which the psychological methods of all schools of psychology could be made explicit. In other words, it looks as though radical behaviorism was a necessary evil before the full fruits of an operational behaviorism could be realized (94). And finally, but by no means exhaustively, the biological view enormously accelerated studies of child development, not in the speculative manner of the psychological movement in education, nor even by the more faithful biographical method, but by the methods of intense experimental control (38, 39, 40, 73).

Altogether, then, it would appear that the facts and fictions which comprise the notion of geneticism have exercised a pervading and wholly seminal influence on the orderly growth of the science of the mind. In spite of this fact, it must be asserted again that neither the several systematic formulations of the science, nor the uninspired eclecticism, have known what to do with the developmental point of view. They have sought to add it to the structure of existing facts and fundamental frames of reference when, as current efforts toward systematic construction fully reveal, they should have used it as an occasion for a complete reorganization and redefinition of concepts. As events have fully proved, it was to become a strong incentive toward fresh statements of method and subject matter. In fact, geneticism has now become one of the prime elements in the passage that must be made from the varieties to the system of psychology. The early attempts to darwinize its legacy of facts and principles, inherited from a wholly different intellectual climate, were no

more successful than the attempt to newtonize the facts which underwrote the theory of relativity.

II. GENETICISM AND INTROSPECTIVE PSYCHOLOGIES

Three instances which suggest the full measure of these considerations are worthy of note. The first is the mode of incorporation of the genetic point of view sought by the introspective psychologies. In the chain of events from Fechner and Wundt to Titchener, psychology became an analytical science of the contents of the mind regarded as dependent on the experiencing individual (16, pp. 377 ff.). This choice was in explicit opposition to Brentano, and to a venerable tradition about the acts or functions of the mind (90). As will be observed below, an act psychology, when stripped of its theological history and metaphysical mode of justification, lends itself readily to the genetic point of view, but the notion of mental contents, or, as Sorokin describes it, the 'individual culture area' (79, p. 98), first secured by a rejection of the difference between external and internal experience, and then justified by the difference between mediate and immediate experience, made psychology almost wholly a matter of self-inspection. To be sure, its data might become public by the intervention of those carefully controlled language responses called introspections (87, 88, 89); but the language responses referred to 'mental processes' which, in lineage, and in their highly personal character, directly stemmed from the rational definition of the mind. This definition, in its turn, made the notion of mental content almost exhaustive of the meaning of the word mind. Even the faculty psychology was a derivative of power or agency found in content, for many of the faculties were named after the kinds of contents worked with, and not after manners of working themselves. When the biological doctrine of evolution, with its concepts of variation and natural selection, was added to this scheme, a problem of major proportions arose, viz., how can an experimenter pass from the observation of his own immediate experience to the immediate experience, that is to say, to the contents of the

minds of other creatures, and especially of the lower animals, of small children, and of the insane.

The device that was used to solve this problem is now a familiar part of psychological literature. As a special case of the broader principle of extrapolation, it was argued that access to the mental lives of other animals could be had by the method of analogy (10, Chap. XX). Basic to the method were the two principles of genetic continuity and of psychophysical parallelism. The first of these principles stemmed from biology, and not from any pertinent psychological facts. The second was mostly an heuristic principle required by the definition that had already been established governing the subject matter of psychology (86, pp. 13 ff.). If the subject matter were defined as immediate experience regarded as dependent on the experiencing individual, some sort of relation would be required between experience (the observed contents of the mind) and the nervous system. This relation could be described as an item by item affair, and take the form of monism, interactionism, parallelism, epiphenomenalism or double-aspectism (67), or it could be described after the fashion of isomorphic relations between dynamically configured fields (59, Chap. III). As a working principle for the psychological sciences some sort of parallelism has been almost the universal because the most workable choice (17).

The genetic continuity of mental facts, then, was not to be established by direct experimental discoveries. Save for the special case of folk psychology (109), it was ordained by presuppositions regarding subject matter and method. Schematically the method of analogy could be expressed by the formula $M = f(RE \times CN \times BA)$, where M is the animal mind to be discerned, RE is the animal's sensory and motor apparatus, CN is the central nervous system, and BA is the behavioral activity of the organism in its life situation (10, p. 513). This is, of course, a plausible procedure. It draws upon a wide array of facts, each group of which can be established with considerable precision (47). Moreover, it is not too unlike the numerous extrapolations that are familiar to men of science in other fields. There is, however, this

notable difference. Extrapolations in the physical, chemical, and even in the biological sciences, are from one domain of discourse to another of the same kind, the noetic status of the experimenter remaining unchanged (8, pp. 228 ff.). The comparative equation, on the contrary, not only allows but forces the experimenter to pass from a domain established by one view of his noetic powers to a domain wherein his knowledge is ascertained in quite another way. A parallel change occurs in the definition of the subject matter of psychology. During most of its experimental history, the method of introspection has served as a device for describing mental contents. These contents are one portion of what Koffka has called the behavioral as opposed to the geographical environment (58, pp. 27 ff.). The former environment was mistakenly called a portion of the mind, the stream of consciousness or states of awareness. Any system of psychology which is based on subject matter derived by the cleavage of the domain of experience into kinds before the operations have been performed which would show where and why the lines of cleavage should be drawn would be compelled to show why a portion of the environment should be called psychological (96). In spite of the difficulties in the way, a persuasive account of the mental lives of the lower animals was achieved by Washburn and other psychologists of the introspective school (102).

The method of analogy, however, did not supply the operational objectivity that was required if psychology was to include the genetic point of view among its generative principles. The reasons are easily enumerated. Worthy only of passing mention are the hazards of any imaginative construction such as affirmations about the mind of an amoeba, to say nothing of the foetus, must require. More objective but nonetheless significant data are now available (24). More fundamental is the definition of the subject matter to which the genetic method was to be applied. It is first to be observed that, by any ordinary interpretation of operational techniques, the natural data of psychology are performances in contexts (31). This was true even of the introspective psychologies, for the performances in question were verbal replies to precise ex-

perimental conditions (101,105). This is an aspect of the introspective method which is fully demonstrated by Johnson's analysis of the psycho-physics experiment (55), and more systematically expressed by Stevens' notion of the discriminative reaction (81) and Boring's analysis of meaning (18, pp. 222 ff.). Performances, however, may be fractionated into at least two contributing members. There are, on the one hand, the contents of the mind or the *whats* that are employed in the course of adjustment, and on the other, whatever may be meant by the acts, faculties, vectors or functions in terms of which adjustments are mediated. Of these two fractionations, Titchener clearly chose the first. In making this choice, he escaped both the criticism of converting psychology into a political or ideological science, and of asking his subjects to describe a certain class of objects, by an excessive form of analysis which stripped objects of all meaning, and left nothing but their raw feels as the basic psychological data (91). Even this was not the end of the matter for the 'raw feels' became clusters of attributes which, from Titchener's point of view, were attributes of nothing (18, p. 21).

Any psychological science which defines its subject matter in terms of the contents of the mind, or of states of consciousness must deal with a set of *whats* which are to be counted among the members, as Koffka would assert, of the behavioral environment. That some of them are instituted by perceiving, and others by remembering or conceiving, and that some of them find expression in handling actions while others find expression in verbalizations, are not sufficient grounds for the division of objects into physical and mental classes (11). Among the *whats* long called mental are the 'things' perceived in the so-called 'external' world, and all the memories, ideas, concepts, judgments, beliefs, sentiments and other members of the so-called 'thought about' world. From the point of view of a naive realism, all of these items, as Bentley (14, 15), Lewis (65), Tolman (96), Hunter (53), Holt (51), and others have asserted, each after his own fashion, are environmental. They should be numbered among the objects which serve as the instigators to, the means for, and the

goals of, the adaptive performances of living creatures (95). At the human level, and when not reduced, by the methods of abstractive analysis, to raw sensory or imaginal feels, they comprise just the materials which make psychology a political, cultural, economic, aesthetic or some other sort of normative discipline. As memories, imaginative constructs, or thoughts, they are that individually appropriated and possessed portion of information and culture often called the mind. It might be said again that they constitute, in Sorokin's phrase, 'an individual culture area.'

In terms of these considerations, Dewey is certainly right when he designates psychology as a political science (34, p. 29). For certain purposes it might be desirable to write a genetic account of the coming into being of a pattern of culture either through the communal activity of many men (69) or as a phase of the development of one portion of the behavioral environment of a single person; but such an account should not be called a genetic psychology. It might be the history of civilization, or the personal biography of an educated man; but it is no more a genetic psychology than is the functional activity of learning and remembering whereby prospective 'contents of a mind' are mastered in a school, and held secure as an available and enduring fund of knowledge. To be sure, when this portion, or any other portion of the behavioral environment of a person is examined by the methods of abstractive analysis, the logically defined entities called sensations, images and simple feelings secure, no doubt, a speculative value, but as Sander quite directly (77) and Werner by implication (104), have pointed out, the 'elements of the mind' do not become. They simply are. The introspective psychologies, accordingly, did not and could not achieve a genetic point of view of operational dimensions. The basic points of reference from which they stemmed might be darwinized, just as a buggy was once motorized, but the time was to come when geneticism would require that the definition of the subject matter of psychology be altered.

III. GENETICISM AND EMPIRICAL BEHAVIORISM

The second instance of the paradoxical position of geneticism among the systems of psychology stems from what might be called the empirical behaviorisms. For a wide variety of reasons, of which the Cartesian distinction between *res extensa* and *res cogitans*, the abstractive and highly rational methods of scholasticism, the folklore concerning the passions and instincts of the animals, the historically independent domains of human and animal psychology, and the steady undertow of static *versus* genetic description during the days of excessive rationalism in science, are the most important, four categories of behavior have long been utilized to characterize psychological becoming. They are the tropism and reflex, the instinct, the notion of modifiable behavior, and the concept of intelligence. Even before Darwin, it was obvious to most of the natural scientists that some sort of genetic kinship must exist between these terms (74). That each of them had been secured by abstracting certain particulars of behavior from their conditioning and sustaining contexts, and reducing the residues to class concepts, each class acquiring the quality and degree of power it needed in order to produce its characteristic form of behavior, seemed quite beside the point.⁴ Happily for an avowed program of developmentalism, tropisms and reflexes were found to dominate the behavior of the 'simplest' creatures (66), the instincts of the higher animals could easily be counted as 'compound reflexes' (80), or as inherited elaborations of reflexes (37), learning with its associated notion of modifiable behavior was found widely distributed among the immediate ancestors of men (70), and intelligence was generally conceded to be a distinguishing characteristic of the 'highest' animals (82).

The main achievements along the route of psychological becoming, then, appeared to be plain. For Yerkes (110, 111), Hobhouse (48), Bühler (23), Holmes (50), Spencer (80), Angell (4), and even for the early Koffka (57), development consisted of a passage from tropisms and reflexes through in-

⁴ It would seem to lie beside the point because the procedure was then accounted as wholly scientific, as Lewin (61) and Brown (21, 22) have noted.

instincts and modifiable behavior to intelligence. The modification of this pattern wrought by Baldwin (5, 6) in terms of habits and accommodations could not really be counted as a substantial alteration of the logic by which a genetic psychology was to be achieved. Thorndike, also, belongs to the same tradition save that, by the theory of connection-forming (82), all four concepts were counted as variations on a single theme, *viz.*, the number of neural bonds that might be available between sources of stimulus and modes of response at different phylogenetic and ontogenetic levels. To be sure, it might be asserted that the flow of events from tropisms to intelligence implies an emergent change in kind of behavior (71), but for Thorndike, change in kind was directly and solely contingent on change in complexity. In an analogous fashion, the doctrine of reflexes, altered through a whole life span by stimulus substitution, became a one-dimensional type of genetic psychology (9, 75).

The difficulties with these proposals for a system of genetic psychology are immense (28). There is, first, the argument of Cope (29), Ward (98) and Titchener (86, pp. 450 ff.) that, in many respects, the genetic order of mental development might have been just the reverse. The reason lies in the evidence which suggests that the first actions of living creatures were conscious. Non-conscious actions would stand as habit residues left over after the necessity for conscious control had disappeared. This is an interesting criticism of the commonly accepted view for, as will be observed in a moment, Titchener at least was right, but for the wrong reasons. If the word consciousness is given an operational and behavioral definition, that is to say, if it becomes a functional term analogous to Brentano's mental acts (19, 108), Bentley's mental functions (12), Thurstone's vectors of the mind (84, 85), or some of Tolman's intervening variables (95), mindedness may very well be as old as life. The experimental problem, in consequence, would be to trace those sorts of adaptive behavior in relation to conditioning contexts which, by virtue of their contribution to adjustment, ought to be called minded (43). Titchener, however, following a long tradition, could

not elect this operational option. He made the notion of mental contents exhaustive of the meaning of the word mind, and his criticism, therefore, reduces to the scientific worth of the method of analogy. Nothing need be added to the comments above on this method.

More destructive of the genetic program of the empirical behaviorisms is the plain fact that, if developmental continuity is to be established between reflexes, instincts, modifiable behavior and intelligence, a medium of continuity must also be established. In other words, it must be shown that, genetically speaking, reflexes can merge into instincts, that instincts can father modifiable behavior, and that intelligence, assuming that it is a mental rather than a normative term, really belongs to a direct line of descent from modifiability. Each after its own fashion, the definitive objections of biological research against Spencer's doctrine of acquired characteristics (54), the configurational attacks on the methods of analysis by which the field properties of behavior in its context are reduced to reflexes (75, pp. 90 ff.), and the incisive genetic studies of Coghill (27), Carmichael (24), Gesell (38), Werner (104) and others on the progressive individuation of behavioral patterns from a more homogeneous or generalized matrix, abundantly reveal the absence of media of continuity. The net effect of these studies is to show that statically derived concepts like reflexes, instincts, acquired behavior, and intelligence cannot be suffused with geneticism so that they will thereupon represent a developmental series. Such concepts might, for certain purposes, be described as arrested moments of becoming, but if, as has actually been the case, they are derived from quite another realm of discourse, the garments of becoming cannot simply be added to them.

IV. GENETICISM AND CONFIGURATIONISM

The third instance of the paradox of geneticism in contemporary psychology centers around the search now being made by some of the Gestaltists for the genotypical laws as opposed to phenotypical itemizations of behavior (21). As has so long been the case in psychology, here is another ex-

ample of the extent to which the study of the mind has been fashioned after the study of matter (46). Derived from analogies with the gravitational field, and from the organismic trend in biology, there is asserted to be a psychological field or a life space among the dynamic properties of which the laws of behavior are to be discerned (63, 64). Clearly, it would be just as great a mistake to search for the genotypical laws of behavior among statistical fusions of the particulars of behavior as it would to derive the genotypical laws of falling bodies from an average of the stones rolling down a hill-side (62, pp. 30 ff.). The psychological life-space, however, or, as Koffka would describe it, the behavioral environment, belongs definitely to the content notion of the mind. It is a modern version of the tradition that the proper business of psychology is, as Hunter has pointed out, to describe the properties of certain classes of environmental objects (53, p. 285). In contrast to the traditions, however, these objects, both when taken empirically as members of the stream of consciousness, or as particular states of awareness, must not, except at the cost of becoming mental faculties, be endowed with force, power, or agency. Escape from the paradox created by passive mental contents as viewed by Titchener, and active or dynamic adjustive power as viewed by the neo-scholastics, and escape also from the microscopic to the molar properties of behavior, was achieved by the doctrine that the whole field of the organism-in-its-environment, or the whole life space of an animal, is dynamically structured. Percepts and concepts, for example, are not localized forces. When they are looked upon as members of a total life-space, they share the dynamic qualities of the life-space. They behave, and the interacting organism behaves, according to the vectors instituted within the structure of the whole field.

Within the range of these considerations, a new paradox develops, for dynamically structured fields must be structured according to certain abiding principles. Otherwise, behavior would be unlawful, or at least alawful. With respect to the notion of becoming, therefore, the genotypical laws of behavior are essentially static. Like the laws of falling bodies

in a structured physical field, the vectored appetites and aversions are the perduring ways in which behavioral fields are structured. To be sure, according to the configurational notion field-structure itself is not static for it embraces a sort of stable equilibrium among intensely dynamic forces, but the formal properties of the resulting field are static. Just as it would be unlikely that the laws of falling bodies are undergoing genetic changes of the sort that would make them fail of prediction tomorrow, so it would be unlikely that the laws of behavior undergo any change in the life history of the developing person. What is dynamically true of the behavior of the infant in its context must also be true of the behavior of the adult in his context, and what is dynamically true of behavior in a life-space, that is to say, in the behavioral environment, would also be true of neural patterns in their relation to physical stimuli. There is, then, no becoming save that which marks the progressive individuation, enlargement, and reorganization of the behavioral environment in which adjustment occurs. And this, as observed above, is a result exactly to be expected of any system of psychology which defines its subject matter in terms of consciousness, of the contents of the mind, of the association of ideas, of the behavioral environment, or of any analogous manner of reference to the 'objects' which serve as instigators to, guides of, or goals for, adjustive action. Development might be inferred from records at successive age levels of the 'contents of the mind,' just as biological evolution was strongly supported by comparative morphology and histology, but the outcomes of processes are not the processes, as quarrels over the meaning of variation, natural selection, inheritable mutations, and the like, fully suggest.

V. GENETICISM AND FUNCTIONALISM

These considerations show how fateful the issue was between Wundt and Brentano when an experimental science of the mind was becoming a real possibility. Wundt adopted the notion of experience, that is to say, of mental content, and Wundt prevailed. Brentano adopted the notion of men-

tal act, and Brentano was over-ruled. Mental acts were too much like the traditional mental faculties with their self-initiating powers, to stand against what Titchener called 'critical science' (93, Chap. I). Moreover, the dynamic or facultative aspect of British associationism had seemed to end in the blind alleys of cognition, conation, and feeling. But now, and because of the ferment introduced into psychology by the biological notion of becoming, and chiefly through the channel of methodology, the essential meaning of Brentano's position is being recovered. It is most conveniently described as a dynamically and configurationally grounded functionalism. In a verbal way at least, functionalism has been a steady current in Thorndike's doctrine of abilities, even though the abilities he lists are usually named by results, products, or goals to be achieved rather than by minded modes of operation (83). Under the influence of operational techniques of the most objective sort, functionalism is a dominant feature of Tolman's methodological behaviorism for, among the 'intervening variables' required in the explanation of the purposiveness of behavior, there are both the mental contents which are points of departure for, means of, and goals to behavior, and there are also 'traits' or 'capacities' (95). From quite a different angle, Thurstone has found, in the analysis of performances, sufficient statistical reason to propose a set of dynamic factors called the 'vectors of the mind' (84, 85). And finally, Bentley, in the introspective analysis of the ways in which mental contents are mobilized for getting psychological work done, finds reason to believe in such functions as apprehending, inspecting, acting, understanding, and thinking (14, 15).

It may be objected, of course, that all of these events are the last echoes of a faculty psychology (1); but it may also be asserted that they are just the concepts which save psychology from being a political science, or an attenuated branch of sociology. It may be asserted even more strongly that they are just the concepts which are suggested by any operational approach to the problems of human nature, for they are attempts to name the channels through which the resources of

the organism pour in its adaptive activities. They save psychology from being a political science, not by stripping its alleged subject matter of all meaning in order to arrive at constructs which are essentially logical (76), but by naming the ways in which living creatures mobilize themselves in a functional way in order to get mental work done. They are the concepts which naturally issue from operational procedures because, in the attempt to achieve an objective definition of consciousness, or of the *whats* with respect to which adjustments are effected, the *hows* of any adjusting at all must be defined. This is to say that the acts, abilities, functions, vectors, or faculties of experimental psychology are not mind-stuff theories but functional and dynamic characteristics of adjustment. They are developing canalizations of modes of operation of the organism-in-its-environment, and not impositions on behavior from an agent exercising active regency.

It is still too early, perhaps, to give a definitive account, both by specific naming, and by specific number, of the possible mental functions, acts, intervening variables or vectors of a minded sort. One might hazard the guess, however, that the broad areas within which the particular vectors will be found are named by such words as acting, perceiving, attending, emoting, conceiving, purposing, thinking, and selfizing including a variety of possibilities commonly called the mechanisms of adjustment.⁵ That the use of these words marks a hazard is suggested, first, by the fact that each of them is little more than a kind of class name. It may include, therefore, not only many individuated modes of functioning but modes of unlike kinds. In the second place, pathological cases of dys-function like aphasia clearly show that neither the known vectors of the mind, the traditional faculties of the mind, or the identification of intervening variables, describe the order in which failure in functioning commonly occurs (44). Moreover, learning, remembering and forgetting have not been included in the traditional list cited above, and the

⁵ Unhappily, most of the mechanisms commonly cited (22, Chap. IX, 78) are named according to goals or results instead of functionings.

inference is that these words—or at least the word learning—refer to the biography of the functions, namely, to their developmental course over a period of time rather than to functions operationally comparable with the others (12, Chap. V).

In spite of the difficulty of definition, however, the location of the subject matter of psychology in functional terms will admit of a biographical, that is to say, of a genetic treatment. The main reason is that the notions of mental function, of vector or of intervening variables of the order of capacities, are operationally definable notions. They cover the unique types of relations that are established between instigators to action and consummatory responses. They are not an extrapolation downward from men to animals or upward from animals to men. They are, instead, plausible derivations of a scientific method by which the defining characteristics, and the genetic status of these characteristics, of any animal whatsoever, can be discerned. Functionings of the minded sort, like those of a vital and a physical sort, spring from the cardinal conditions of every experimental arrangement. The object under inspection is found to enter into certain kinds of relations with its relevant context. The defining features of behavior qua behavior-in-its-context can be established for any creature at any stage of development without changing the operational position, that is to say, the noetic powers of the experimenter. This is an explicit formulation of what has been implicit even in experiments of the introspective type. Its essence is that the operating nature of a thing is determined by the relations which obtain between it and the probe bodies that are used as the relevant features of an instigating, directing, and sustaining context (107). The subject matters of the various sciences, accordingly, are not discovered by artificially dividing the world into classes. They come by inferences from the ways in which the discriminated members of the field of naive experience perform in relation to the operations that can be carried out on them. Minded behavior is not a state of mind, or a quality of experience, or an order of being but an operationally defined class of performances. These performances are to be com-

pared with vital behavior and physical behavior which are also operationally defined.

A functional psychology, then, or, as Tolman describes it, an operational behaviorism (97), promises to resolve the paradox of geneticism. It does so by virtue of the fact that it places the problem of mental development on all fours with respect to the experimenter and to the laboratory. More than this, however, is accomplished, for a functional psychology, grounded in dynamic and configurational concepts, is not wholly feasible until geneticism becomes more than an addition to existing concepts. It becomes one of the tools for reshaping the fundamental categories of the science of minded performances. It reaches beyond the introspective and the association psychologies because it inquires, through the medium of operational techniques, what mobilizations of resources for functional activity are implied by the 'behavior' of mental contents. It reaches beyond the functional revolt of Angell and Carr because it gives the word 'function' its proper dynamic setting. It avoids the notion of contents which function simply by being present during an adjustment. To be sure, the wider the range of the behavioral environment, the 'wiser' or the more 'useful' the adjustment, but functionalism fell short of the requirements of a functional psychology because it still regarded mental contents as somehow definitive of the word 'mind.' Dynamic functionalism reaches beyond radical behaviorism because this latter was a description of the muscular and glandular resources available for adaptive performance rather than a description of actions in their contexts. It reaches beyond configurationism because observations of the dynamic properties of behavioral fields and of their isomorphic relations with neural fields, are not equivalent to the observation of the dynamic properties to living creatures in their environments. The notion of vectored quality of behavior in a life space, and the notion of the properties of paths, that is to say, hodological notions, are not of sufficient breadth to cover the cross-sectional front of adaptive effort. Even when an instance of behavior is clearly directed toward a goal, the behavior has a cross-

sectional spread which is quite obscured by asserting only its vectorial quality.

VI. GENERAL PRINCIPLES OF DEVELOPMENT

The effects of the systematic reconstruction of psychology brought about by field theory and geneticism are, of course, just as wide as the whole science. For present purposes, however, special attention should be directed to what might be called the genotypical principles of development. This is an important consideration because the absence of such principles has been a distressing feature of all attempts to describe social and cultural evolution, to give functional meaning to educational psychology, and otherwise to understand the various applications that might be made of the time-wise patterning of mental events. Unfortunately, and in spite of the immense amount of work that has been done on all phases of development at all animal and human levels, only the scantiest of suggestions are available regarding possible laws or principles of becoming (49, Chap. XVII). The clearest instances are suggested by such words as directedness, continuity, progressive individuation, serial order, varied tempo, optimal realization, organic quality, and increasing inertness (42, pp. 108 ff.). These words are to be taken as meaning only that, in their end-to-end wholeness, growth patterns reveal certain features, qualities or time-wise dispositions that are just as significant to the system of psychology as are the dynamic or configural properties of behavior in its specious immediacy. They are an increasingly rich by-product of attempts sufficiently to characterize growth schedules, as especially exemplified by the exposition of Barker (7, pp. 13-28).

The directedness in the becoming of patterns of behavior clearly involves the twin terms of heredity or maturation and learning. The better because more embracing complement to maturation, of course, is the concept of environmental pressures. In spite of varied rates of growth which, as at birth or adolescence, give the appearance of saltations, and are sometimes called trauma, almost all students of genetic

psychology argue for essential continuity through an irreversible serial order. Even the behavioral explosions which lie beneath the psychoanalytic view, argue for continuity in becoming. The most significant principle, and a crucial test of the systematic position of geneticism, lies in the principle of progressive individuation. Whether it takes the form of the emergence of figures from grounds (57, pp. 130 ff.), the differentiation and specialization of motives and of emotional actions (20), the inspection and discriminative analysis of the properties of objects and of their relations (81), the progressive individuation of particular skills out of mass actions (45), or the passage from the syncretic to the specific (104), the differentiation and specialization of the members of a growth pattern are a rapidly unfolding story to students of becoming (27). So, too, is the extent to which varied tempo and progressive individuation depend on qualitative characteristics of environmental pressures. There is, in short, a kind of law of least action (106, pp. 82 ff.) which, for psychology, might better be called a principle of optimal realization with respect to given contexts. In spite of progressive individuation, however, and as a consequence, in part, of optimal realization, the whole spread of a growth pattern, that is to say, the whole array of its constituent members, displays a sort of organic quality whose closest analogue, perhaps, is found in the science of ecology. This organic quality is not a matter of perfect correlation between individuated abilities, but a kind of compensatory supplementation by virtue of which the organization and mutual dependence of the whole functioning person is established. Even the adjustive psychologies, in their definition of compensatory behavior, imply that the goals used as compensatory belong to an order, and serve a personal economy, which clearly suggest organic quality.

The consequence of all these characterizing trends in growth patterns is the emergence of an individual. Individuality means the progressive attainment of higher degrees of self-initiative, self-government, and self-regulation, namely, the attainment of a kind of closure of the self. It amounts almost to a psychological equivalent of homeostasis, and the

achievement of a sort of personal power. Such an individual becomes increasingly resistant to change. Stated the other way round, he becomes decreasingly permeable to environmental pressures. Because of their remarkable resistance to change, the genes of such a person could be said already to have achieved this status. At least the biological factors which lead to physical and neural growth far exceed personalities in their closure to outside agents. It is characteristic of mental growth, accordingly, that inertness in belief and opinion, or loss of plasticity in learning, or increasing stability in interests and motives, do not occur until middle life. Most of the individualities of the physical and biological worlds represent a high degree of independence and closure; but the gradual achievement of these traits, both in a content and in a functional sense, is just what makes the educative process a central concern to the human race.

VII. SUMMARY

In summary, then, geneticism has long occupied a paradoxical position among the isms of psychology. It now appears, however, to stand at a turning point for the system of psychology. The paradox could not be resolved simply by adding newly discovered concepts and points of view to principles fashioned by a mode of thought which had repudiated geneticism in the first instance. The developmental point of view carried in its train a new type of experimental zeal for the study of behavior. The study of behavior, in its turn, and supplemented by a whole host of other considerations, made manifest the operational methods which have been implicit in, and practiced by, psychologies of every sort. Before the advent of the laboratory, many of the more zealous students of human nature passed all too quickly to syntactical propositions about the meaning of their data. These statements, varying in range from a rational mind to the flow of experience viewed as dependent on the nervous system, appeared to be the proper subject matter of psychology; but this was a situation that could not endure in the face of the concepts loosed by geneticism. Even the

behavioral attempts to write an objective definition of consciousness placed emphasis on methodology. This was a gain in that behaviorism, having suggested the reconstruction of psychological principles, brought forth a definition of subject matter whose cross-sectional and longitudinal aspects could be viewed at one and the same time, and to the benefit of both. The subject matter of psychology is becoming mental functions, and the biography of mental functions, established by experimental methods, supplies the data for a genetic psychology.

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PSYCHOLOGY'S PROGRESS AND THE ARMCHAIR TABOO¹

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If we view the history of American psychology since the 1890's, when the American Psychological Association was founded, as a broad culture complex, we can detect the emergence at about that time of our enthusiasm for the totem of experimental techniques. This enthusiasm has become an established, orthodox tradition to which we all pay formal homage. We are proud of this totem. Even those of us who through indolence, administrative responsibilities, paucity of creative ideas, or lack of initiative have not made any sacrifices to this totem since our thesis was published, would hasten to repudiate any suggestion that our failure to sacrifice means loss of faith in the potency of the totem. We would reenforce our repudiation by references to the laboratory courses we direct, the experimentally grounded theses we supervise, and our lip worship to the experimental gods as exemplified by the rigorous orthodoxy of our classroom lectures. Should the skeptic still harbor lurking doubts of our devotion to the official totem, we might even be constrained to furnish samples of the sincerity of our lip worship. Among these samples we might lay especial stress on the vehemence with which we eschew 'armchair psychology.' If experimentalism is our totem, armchair psychology is our taboo. Even amateur anthropologists know that not all taboos are indicative of intelligent prohibitions. Some of them may be superstitious survivals of practices whose logical justification is entirely a matter of history. Others survive as verbal shibboleths due to the prestige of the elders of the

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tribe or to the kind of inertia responsible for the anachronism of cultural lags.

What kind of a taboo is our armchair taboo? Is it up-to-date or is it an anachronism? Does it still serve our science or has it outlived its usefulness? To the extent that one can do so within the constraints of a brief paper, we shall plead for emancipation from the shackles of this taboo. We shall endeavor to indicate that, far from making for the progress of psychology, continued allegiance to this taboo functions as an obstacle to the advancement of our professional work.

A few words regarding the origin of this armchair taboo might well be introduced at this point. The taboo emerged about 1895 at the time E. W. Scripture was instructor at Yale under the aegis of George Trumbull Ladd. Scripture himself coined the phrase 'armchair psychology.' It may prove helpful in the present context to recall what Boring (1, p. 514) has to say about Scripture in the following passage:

He was a great contrast to the theological and philosophical Ladd, coming into the laboratory with a strong conviction as to the scientific nature of psychology and its mission to work quantitatively upon the mind, ever approaching more and more nearly to the precision of measurement that obtains in physics. . . . (His) books still carry the fervor of the '90's: a *new* psychology, soon to be as accurate as physics!

With Ladd as senior professor it is not unlikely that Scripture, the young Wundtian enthusiast, intended his warning about armchair psychology to be a verbal thrust at the academic armor of his superior. At all events, it was seemingly more closely related to the theologically and metaphysically oriented psychology of Ladd than to Wundt's total system of psychology. We mention this because Wundt himself was by no means persuaded that the experimental approach was the exclusive scientific one for psychology. He, it will be recalled, was of the opinion that the complexities of the higher thought processes were not amenable to experimental prosecution. As a substitute for experiment he sponsored the kind of methodological attack now associated with the work of the cultural anthropologist. The dreary

volumes of Wundt's *Völkerpsychologie* constituted Wundt's own non-experimental contributions to his conception of psychology as science. It is to be doubted therefore whether Scripture ever intended his aversion to what he dubbed 'armchair psychology' to rule out all non-experimentally established data. It was intended as a slogan to win converts to the new psychology of the period and away from the empirically non-verifiable metaphysical presuppositions still operative among many of Ladd's philosophical contemporaries. Scripture (6, p. 2) himself granted that 'simple observation of our minds' can at least give us 'general outlines of facts.' However, as many post-Scriptural psychologists seem to have interpreted the phrase, the armchair taboo was to make psychology a 100 per cent laboratory science.

At the turn of the century there were enthusiasts who envisaged the 'new' psychology as a rigid laboratory discipline potentially entitled to the intellectual respect being accorded physics and chemistry. The beginner in psychology, in some places, was trained to avoid any references to mental life or human nature, no matter how confident he might be of their accuracy and cogency, unless he could cite some experimental basis for them. The empirical approach was thus being narrowly restricted to the experimental approach. All non-experimental data were stigmatized as armchair stuff. And this, despite the fact that much of the scientific work in established subjects like botany and zoology and astronomy was non-experimental. Darwin's field work, for example, was more empirical than experimental; but this fact did not induce his fellow-biologists to make short shrift of his observations by a cavalier reference to armchair biology.

We have long been puzzled by an anomaly in the early history of 20th Century American psychology. As a group these American founding fathers were rallying around the totem pole of laboratory psychology and proud of their loyalty to the armchair taboo. As a group they were teaching that the only way to know and understand mental life or behavior or consciousness—the only way to achieve professional competence as a psychologist was to immure oneself in the lab-

oratory. And yet when Cattell induced them to evaluate the relative eminence of the distinguished psychologists of the period they agreed, as we recall it, in voting William James the number one American psychologist. Years later when Tinker (8) and his associates duplicated the Cattell technique American psychologists were still ranking James in first place. What ought to be stressed, though, in the present context is that not even the most enthusiastic admirer of James could classify him as an experimentalist. A college junior completing one of our conventional year's courses in experimental psychology very likely does more laboratory work in nine months than James did in a lifetime. Nevertheless twice in the history of American psychology ardent devotees of experimentalism acknowledged the professional superiority of William James, the armchair psychologist. This is tantamount to admitting that one can become an expert in the field of mental phenomena without exclusive preoccupation with laboratory problems.

What made James a great psychologist? It could hardly have been his familiarity with experimental literature to which he contributed so little and for which he seems to have had only a rather nebulous and somewhat latent respect. Even this grudgingly granted incipient respect may sometimes have come very close to bored contempt. Certainly a phrase like 'brass instrument psychology' suggests a somewhat negative attitude toward experimental work. His greatness, as we see it, was due to his empirically grounded insights into the richness and diversity of mental processes as they are experienced. James was an empiricist but not an experimentalist and his empiricism taught him more than experimentalism taught many of his colleagues. He was an armchair psychologist with his chair planted in the pulsating world of experience and not in the arid atmosphere of wordy metaphysical abstractions.

What we are urging is not that we have a moratorium on experimentation. We are in favor of more revealing and more significant experimental work in psychology. But in addition, we are for the frank recognition of the extent to which

our science can be enriched by armchair empiricism of the William James variety. Our students should be trained to be both experimental and empirical. To glorify experimentalism and to disparage empirical insights as 'armchair stuff' is to blind them to much that is vital and relevant and stimulating and true. Consider, for the sake of illustration, the following passage from Stout's *Manual* written before Wertheimer's contribution of the phi-phenomenon enables us to enlarge our vocabulary of psychological abuse by calling people 'atomists' or 'associationists' or 'reflexologists.' In other words, even before the experimental work of the Gestaltists furnished us with the dictum of the priority of the whole, Stout (7, pp. 151-152) had already written:

A complex whole as characterized by its specific form of unity has attributes which do not belong to any or all of its parts; and inversely the parts may have attributes which do not belong to it. A heap of stones may be a pyramid, though no single stone is a pyramid; each stone may be round though the heap is not round. . . . A triangle is a closed figure. But its lines or angles are not closed figures. It is above all important from the psychological point of view that a whole object in its unity has a distinctive function and value as a factor in mental process, different from that of its parts. A melody yields a pleasure which is not due to its component tones considered apart from their union.

If we are enamoured of configurational teachings then we ought to be enamoured of this pre-Gestalt teaching of Stout's. Stout was not an experimentalist, but this does not necessarily invalidate the cogency of his empirical observation. Psychology is richer because he made it.

A second example may be helpful in that it will serve to show that armchair observations of psychological value may be made by those who are not professional psychologists. Almost every recent text in social psychology devotes substantial space to the concept of social stereotypes. Britt's (2) work, which was published only this year, has almost fifty references to stereotyping. Who called our attention to the existence of this phenomenon? Was it a Titchener, a Thorndike, or a Pavlov? It was a journalist and a student of

politics: Walter Lippmann. If stereotypes are the ubiquitous and pernicious phenomena we now recognize them to be, why did we presumed experts in the field of human nature have to wait for a journalist to come along and shock us into a realization of their existence? My provisional answer is that we had trained our early social psychologists not to look for such phenomena. We endowed them with a phobia against critical observation of their own mental processes—and a social stereotype is a mental process—by excoriating such observation as unscientific introspection or worse yet, armchair psychology. Instead we indoctrinated them to seek scientific respectability by sticking to the language of chain reflexes, J-curves, synaptic resistances, and, in our more sadistic verbal moments, phrictopathic sensations.

By way of fortifying the general drift of this appeal a third example might be introduced. Almost all psychologists—even those most resolutely opposed to psychoanalysis because of its non-experimental nature—warn their pupils of the treacherous consequences of rationalization. Well, who taught us that such a process as rationalization exists? It was not a laboratory man, but Ernest Jones, the British disciple of Sigmund Freud. Why do we talk and write as if the process of rationalization were a scientific reality if we are committed to the dictum of repudiating all armchair contributions? Is it not because, once an armchair observer has called our attention to the existence of the process, we find such overwhelming confirmation in our own observation of our own thinking and in that of others that we are quietly compelled to acknowledge its existence? It becomes an empirically grounded fact of such cogency that to insist on experimental confirmation would be as fatuous and as superfluous as to refuse to believe that more men than women sing bass because nobody has proved this by laboratory tests or field investigations. Incidentally, it might also pay us to reflect on the possibility of a conflict between an empirical and a laboratory conclusion. The history of science ought to warn us against accepting the latter as invariably and necessarily more trustworthy than the former. Stumpf and

Wundt, it will be remembered, had this kind of controversy, with Stumpf insisting that he, as a musician, knew what Wundt had to report on the matter of tonal distances as studied in the laboratory could not be squared with the empirically grounded knowledge of the musical experts. It might also be well to reflect on the truism that careful empirical observation may often contribute more to a field than a carelessly executed experiment. Or, to put this differently: Good armchair psychology is preferable to sloppy experimentation.

We have already given some samples of good armchair psychology. Many others might be added. Freudian observations involving the existence of defense mechanisms, projections, displacements, and the concept of identification could be incorporated in a complete list. As has already been implied, even the most bitter anti-Freudians among us have found ourselves compelled to tacitly absorb some of the descriptive vocabulary this non-experimental school has introduced. What is more, some of the Freudian insights based on shrewd empirical observation are receiving experimental confirmation. Levy's (4) study of behavior differences in dogs as determined by the ease or difficulty of their nursing experiences during puppyhood is a case in point. So is Hunt's recently published study of hoarding behavior in the rat. In this connection it is relevant to introduce part of Hunt's final sentence in which he says: "These results tend to substantiate the psychoanalytic claim that infantile experience is an effective determinant of adult behavior" (3, p. 359).

For still one more example of what we regard as good armchair psychology Maslow's analysis of the distinction between deprivation and frustration might be mentioned. His differentiation between mere deprivation and deprivation symbolic of loss of prestige or social effectiveness is exceedingly valuable. In his own words:

Neglect of this distinction has created a great deal of unnecessary turmoil in psychoanalytic circles. An ever-recurring question is: Does sexual deprivation inevitably give rise to all or any of the many effects of frustration, *e.g.*, aggression, sublimation, etc. It

is now well known that many cases are found in which celibacy has no psychopathological effects. In many other cases, however, it has many bad effects. What factor determines which shall be the result? Clinical work with non-neurotic people gives a clear answer that sexual deprivation becomes pathogenic in a severe sense only when it is felt by the individual to represent rejection by the opposite sex, inferiority, lack of worth, lack of respect, or isolation. Sexual deprivation can be borne with relative ease by individuals for whom it has no such implications (5, p. 365).

This leads to our final hypothesis, that perhaps frustration as a single concept is less useful than the two concepts which cross-cut it, (1) deprivation, and (2) threat to the personality. Deprivation implies much less than is ordinarily implied by the concept of frustration; threat implies much more (5, p. 366).

Here we have a superb example of sound armchair psychology. It is reminiscent of the kind of analysis William James might have made. It suggests the type of keen observation of mental life our armchair taboo has tended to discourage. Without belittling experimental work we might now be justified in encouraging our students to indulge in armchair reflection of a serious sort. We need no longer worry about the feelings of inferiority of our psychological forbears because of the greater precision and rigor of the natural sciences. The hazard of being regarded as metaphysical dreamers no longer constitutes a genuine threat to our academic respectability. We need not even be concerned about being stigmatized as philosophers. Far too many of our graduate students and teaching colleagues are too uninformed of technical philosophy to render this a serious consideration.

In other words, all of the factors responsible for the introduction and acceptance of the armchair taboo are no longer operative. It is stupid to continue to be handicapped by such an incubus. It is to be hoped that, once we become freed from it, there will be a facilitation of work in psychology. The more students are taught to study experience—the more they are encouraged to follow such keen psychological observers as Stout and Freud and James—the greater the likelihood of enriching our science by more such valuable insights

as the priority of the Gestalt, rationalization, and social stereotypes. These and many others like them are armchair contributions smuggled in despite the taboo. By removing the taboo we can avail ourselves of the possibility of an increase in both the quality and quantity of such valuable intellectual commodities. Free trade is healthier than smuggling. Empirically grounded armchair psychology is healthier than flights into the abstractions either of abstruse statistical speculations or hypothetical engrams. This ideal of a healthy empiricism does not involve a mutually exclusive, dichotomous choice. It is not a question of armchair psychology *versus* experimental psychology. On the contrary, it calls for the recognition of the essential compatibility of both approaches, their interdependence, and their value for the progress of psychology as a whole. There is no longer any basis for the laboratory man's phobia of the armchair. It might even enhance his efficiency if he could be induced to accept a change in his official furniture by consenting to draw up such a chair alongside his laboratory table.

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A THEORETICAL INTERPRETATION OF SHIFTS IN LEVEL OF ASPIRATION¹

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SUMMARY OF EXPERIMENTAL PROCEDURE AND RESULTS

In experiments aimed to discover the factors influencing the level of aspiration two sessions, separated by about a week, were held with each subject. The subjects were all college students mostly in their sophomore year. In each session a measure of the level of aspiration of the subject was obtained. The measure obtained was expressed in terms of the difference between the subject's performance and his estimate of future performance. This measure has been called the discrepancy score. Three experimental variables were introduced: (a) Reality-Irreality variable.—In order to elicit the estimate of performance for the next trial half of the subjects were asked, "What score do you expect to get next time?" The others were asked, "What score would you like to get next time?" These have been called the 'expect' (reality) and 'like' (irreality) groups respectively. (b) Group variable.—This variable was introduced during the second experimental session. One third of the subjects were compared to a high school group, one third to a college group and one third to a graduate student group. (c) Position variable.—This variable was also introduced during the second session. Half of the subjects were told that they were scoring above the average of the comparison group. The other half were told that they were scoring below the average of this other group.

The three variables cut across one another so as to form twelve distinct experimental groups of equal size. What

¹ This is the second of two articles reporting an experiment done as a thesis for the degree of Master of Arts. The first article (3) presented the results in full. Indebtedness for aid and advice is again expressed to Dr. Kurt Lewin.

these twelve groups are can be seen by the headings along the abscissa in Fig. 1. (Expect, above high school; like, above high school; expect, above college; etc.)

Figure 1 presents most of the results necessary for an understanding of the relevance of the explanation which forms the main body of this paper. The white bars show the changes in discrepancy score from the first to the second session for all the 'like' groups. The shaded bars give the corresponding data for all the 'expect' groups.

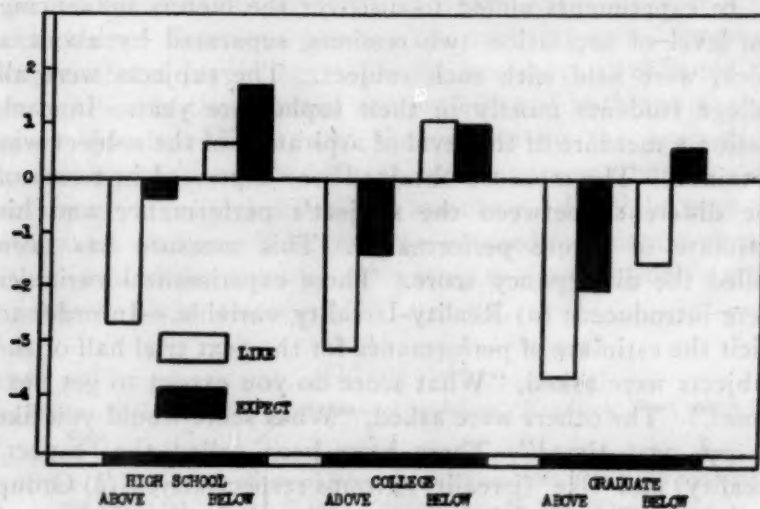


FIG. 1. Changes in discrepancy score from first to second session.

In the expect experiment all the above groups shift their discrepancy score downward, while all the below groups shift their discrepancy score upward. The trend in the magnitude of shift for the above groups is the inverse of the trend for the below groups going from high school to graduate student comparisons.

In the like experiment the direction of the shift is the same as for the expect experiment except for the below graduate student group. This is the only below group that shift their discrepancy score downward. In extent of the shift, unlike the expect group, the like group shows an increasing

magnitude of shift with increasing status of the comparison group for the above and below groups alike.

The expect and like groups differ in certain other respects. The discrepancy score for the like group in session I is significantly higher and more variable than for the expect group. The changes in discrepancy score from the first to the second session are also higher and more variable for the like than for the expect group.

We shall now attempt to explain these results by first developing a theory of level of aspiration and then seeing if the implications of our theory are consistent with the results of this experiment.

THEORETICAL DISCUSSION

Let us consider the case of a hypothetical subject 'A' who has been scoring about 8 on the average in a series of tests. His level of aspiration has hovered about the value 9. While actually scoring 8 he has this goal of 9 which he expects to reach. When he gets a score of 9 he feels a certain measure of success in having reached what he is aiming at.

'A' is now told that on this test where he scored 8 a group of other college students scored only 6. He is scoring above the average of this group. Up to this point he has considered a score of 8 as only fair, and a score of 7 was rather bad. Now he finds out that a score of 7 is good, in fact a score of 6 is an average score with which he might very well be satisfied. There is no longer any point in striving for a score of 9 when a score of 7 is a good one to get. It may possibly seem even a bit foolish to keep on trying to get 9. As a result, 'A' lowers his level of aspiration to 8. He now merely wants to maintain his present position.

However, suppose that 'A' had been told that the average score for the college students was 10. The situation would be quite different. A score of 8 is definitely a bad score. Even a score of 9 which he had previously looked forward to is a bad score. 'A' wants at least to do as well as the group. He therefore sets himself a new goal of 10.

'A' changed his behavior in different ways as a result of

different changes in his situation. The essence of these situation changes was that 'A's' idea of what constituted a good score and what a bad score was changed. When told what others had scored on these tests the attractiveness and unattractiveness of various levels of achievement are changed for him. In short, the change in the situation produced a change in the valence of regions of performance.

We can state then that the level of aspiration is a function of how desirable it would be to succeed at a certain level of performance: a function of the positive valence of success.

It seems to 'A' that a score of 10 is more desirable than a score of 8. It is certainly more desirable to get 12 than to get 10. The positive valence of success, then, seems to increase with the level of difficulty.

'A' does not, however, set his goal at 12 or 14 although success at these levels would undoubtedly be more desirable than success at 10. It will be disagreeable to try for something and not get it. We can then state that the level of aspiration is also a function of how disagreeable failure would be at the different levels of performance: a function of the negative valence of failure.

It is more disagreeable to 'A' to try to get 10 and fail than to try for 12 and fail. It is even more disagreeable to fail while trying for 8. The negative valence of failure therefore decreases as the level of difficulty increases.

If this analysis is true we would expect 'A' to choose the highest possible level of performance as his goal since here the positive valence of success is highest and the negative valence of failure is lowest. 'A' does not do this. Escalona (2) suggests the following explanation. It is doubtful whether the desirability of success and the undesirability of failure at a given level of performance are both present in the life space of 'A' with equal effectiveness at all times. The desirability of success is uppermost in his mind if success is most probable, while the undesirability of failure is uppermost in his mind if failure is most probable. It is then the subject's expectancy of success and failure at a given level of performance which will define the relative potency of the valences of success and

failure at that level. At easy levels the probability of success is very high and so the potency of the positive valence of success will be great and the potency of the negative valence of failure correspondingly small. The opposite will hold for the difficult levels of performance. We now see why 'A' does not place his level of aspiration at the highest possible point.

We have distinguished four factors which influence the choice of a goal: the positive valence of success (Va_s), the negative valence of failure (Va_f), the potency of success (Po_s), and the potency of failure (Po_f). The choice of goal region (L), that is to say, the level of aspiration, will be determined by the resultant force toward L , the strength of which depends on these four factors. This resultant force (f^*) for a given level of difficulty may be determined by the equation:

$$(1) \quad f^*_{p,L} = Po_{s,L}(Va_{s,L}) - Po_{f,L}(Va_{f,L})$$

That region (L) toward which f^* is greatest will be chosen as the goal region.

(2) Level of aspiration = L at which $f^*_{p,L} = \text{maximum}$

In a task which presents a range of difficulty, these four factors may have values at each point in the range. Each of these factors may then be represented by a curve extending through that part of the difficulty range for which that factor has a value. We shall now more adequately define these four factors:

(1) Valence of success (Va_s) is defined as the positive valence of future success as it appears to the subject when setting his goal. This valence would be very low, or perhaps zero at the very easy levels and would rise to a maximum at the difficult levels of performance. It is probably an S shaped curve because the areas of too difficult and too easy performance seem only slightly differentiated with regard to valence.

(2) Valence of failure (Va_f) is similarly defined as the negative valence of future failure as it appears to the subject when setting his goal. This curve would be high at the easy levels and low at the difficult levels.

(3) Expectancy of success (P_{0s}) is defined as the judgment of the individual at the time when he sets his goal as to the probability of reaching a given level of performance. This curve would be high at the easy levels (the individual would feel sure he could score at least that much) and would be very low at the difficult levels (the individual would be sure he could not score that much). The maximum value of this curve (practical certainty) is unity.

(4) Expectancy of failure is correspondingly defined as the subjective probability of failure at the time of setting the goal. Mathematically the $P_{0f} = 1 - P_{0s}$. Psychologically this is not necessarily so although on the whole it is approximately correct: the expectation of failure decreases as the expectation of success increases.

The curve of the resultant forces which will determine the choice of goal region may be derived by formula (1). The region at which this curve reaches its maximum will be the level of aspiration.

Figure 2 is an illustrative example of the derivation of such a resultant curve from the curves of the four factors above presented. The abscissa represents different regions of difficulty and along the ordinate are arbitrary units in which the valences are measured. The curve for the potency, of course, has different units along the ordinate, but the two have been superimposed. The curve for the potency of failure has not been represented since all along it is merely 1 minus the potency of success curve. The point at which the resultant curve reaches its maximum (indicated by the arrow) is the point at which the level of aspiration is set.

Let us now discuss this curve analytically with a view to understanding the way in which it depends on the four factors. In this curve should be distinguished: (a) the maximum point; (b) the gradient away from this maximum and the amount of difference within the curve; (c) the height of the curve above the zero point.

The height of the V_{a_s} and the V_{a_f} curves will depend upon how intensely the individual in question feels about future success and failure respectively. For an individual who feels

success very strongly and failure rather weakly, the former curve will be high and the latter low. The reverse situation may also be the case. This lowering and raising of the heights of these curves does not affect the point at which the resultant force will reach its maximum *as long as the slope of the curve*

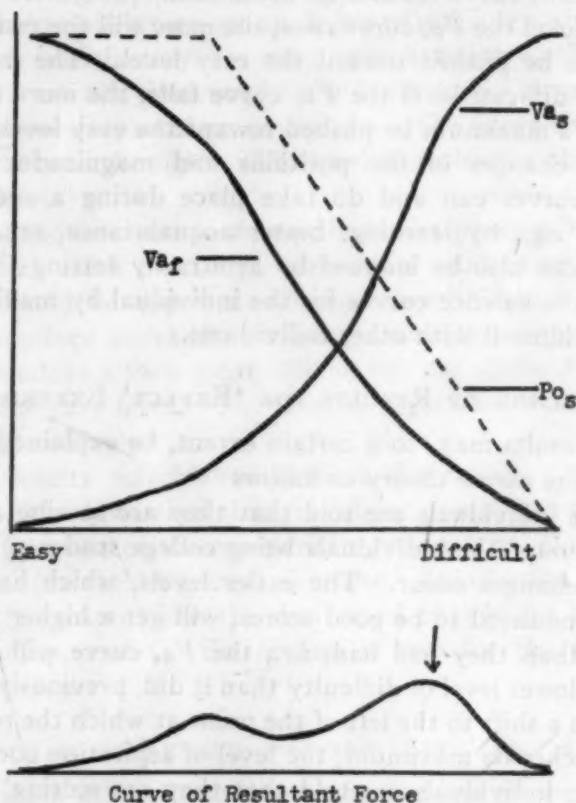


FIG. 2. Derivation of the resultant force ($f^*_{r, L}$) from a set of valence and potency curves of given value.

is not altered. It does, however, affect the height of the resultant curve. The higher the Va_s curve, the higher will be the resultant curve; the higher the Va_f curve, the lower will be the resultant curve. The height of the resultant curve at a given point shows the strength of the force toward this difficulty level (L). This resultant force will be positive if

the failure factors ($Va_f \cdot Po_f$) are less than the success factors ($Va_s \cdot Po_s$), since $f^*_{P,L} > 0$. In the reverse case, $f^*_{P,L} < 0$, or $f^*_{P,-L} > 0$: there is a tendency for the subject away from L .

Changes in the position of the valence curves along the abscissa and changes in the slope will affect the point at which the resultant curve reaches its maximum. The more toward the easy level the Va_s curve rises, the more will the resultant's maximum be pushed toward the easy level. The more toward the difficult level the Va_f curve falls, the more will the resultant's maximum be pushed toward the easy level.

Such changes in the positions and magnitudes of the various curves can and do take place during a succession of trials, *e.g.*, by learning, better acquaintance, etc. Such changes can also be induced by arbitrarily setting the positions of the valence curves for the individual by making him compare himself with other individuals.

DERIVATION OF RESULTS FOR 'EXPECT' EXPERIMENT

Our results may, to a certain extent, be explained on the basis of the above theory as follows:²

If the individuals are told that they are scoring above a college group (the individuals being college students) certain valence changes occur. The easier levels, which have now been pronounced to be good scores, will get a higher positive valence than they had had, *i.e.*, the Va_s curve will start to rise at a lower level of difficulty than it did previously. This results in a shift to the left of the point at which the resultant force reaches its maximum; the level of aspiration goes down.

If the individuals are told that they are scoring below a college group the opposite effect is obtained. Scores which were previously considered good are no longer attractive. The curve shifts to the right and the level of aspiration goes up.

If these induced standards are not of one's own group, the status of the other group with respect to the individual will be important. If the other group occupies a lower status

² In the following derivation it will be assumed that the individual's judgment of his own ability (the probability curves) remains the same and that the Va_f curve likewise remains unchanged.

(*e.g.*, a high school group), being above them produces little change in the curve's position. The subject feels that his own standards should be higher than those of the high school group. Being below a group of lower status should produce a change of even greater magnitude than a similar position with respect to one's own group. In this case similar to the situation of scoring below one's own group, levels of difficulty which were previously satisfactory are no longer attractive. The fact that they are not even good for a lower group should make their valence drop even more sharply. The curve then shifts considerably to the right. The level of aspiration should rise accordingly.

If the induced standards are those of a group with higher status than one's own (*e.g.*, a graduate student group) the effects should be opposite. Being below this higher group should produce no change. Being above this higher group should produce a very great shift in the rise of the V_a curve toward the easy level. The level of aspiration should again behave accordingly.

The results coincide with these derivations. Figure 1 shows (1) the downward changes (above group) increase with increasing status of the comparison groups, and, (2) the upward changes (below group) decrease. In addition, (3) the upward is greater than the downward change for the high school group, and (4) the reverse is true for the graduate student group.

DERIVATION OF RESULTS FOR THE 'LIKE' EXPERIMENT

The main difference between the like and expect experiments is that the subjects of the former behave on a more unrealistic level because of the emphasis on wish rather than expectation in the instructions. The chief differences between the levels of reality and irreality (5) are that the level of irreality presents a more fluid medium. Locomotion through barriers is easier than on the level of reality. Because of fewer restraining forces on the level of irreality, the response to equal driving forces would be greater on the ir-

reality level than on the reality level. On the level of ir-reality then, the individual can structure his life space in accordance with his wishes. If this is true, then the like category is less bound by considerations of reality in achieving success and avoiding failure than the expect category which behaves realistically.

This theory points to the following conclusions: (1) The discrepancy between level of aspiration and performance should be greater for the like group than for the expect group. (2) The amount of change in discrepancy score after comparison with other groups should be greater for the like group. (3) The like group should show more latitude in accepting or rejecting the standards of other types of groups. (4) The like group should show more freedom in accepting or rejecting the same group standards under different conditions in line with their wishes.

Conclusions (1) and (2) are found to be true. The like group does have a higher mean discrepancy score and a higher mean change in discrepancy score than the expect group. Conclusions (3) and (4) will be discussed below.

We have seen that the choice of a goal level was largely guided by the negative valence of failure and the positive valence of success. This tendency to avoid failure and achieve success is present throughout and may lead, for example, to rationalization after the action (blaming the tool). This tendency has been reported frequently by other experimenters and is one of the basic facts in a level of aspiration situation.

Another way to avoid failure or achieve success in our experimental setting lies in accepting or rejecting the standards of the comparison group. For example, a child in school who continually gets grades far below the group might accept his position as low and reject the group's standards as binding for himself.

Such rejection of the standards of one's own group seems to be rather difficult. A person has more latitude, however, in accepting or rejecting standards of some group other than his own. An adult will usually refuse to see his actions judged according to the standards of preschool children, both in a

positive and negative sense. Experiments have clearly shown that the areas of 'too easy' and 'too difficult' lie outside the region in which success and failure are felt.

In the light of the above elaboration of our theory, the results of the like group become compatible with those of the expect group. The acceptance of a position below a high school group means admitting failure which the like group can avoid by refusing to compare themselves with the high school group. Thus the V_a curve remains relatively unchanged.

In the same wishful manner the like group accepts the standards of the high school group when placed above it, since such acceptance means success. The level of aspiration accordingly shifts downward. The more realistic expect group cannot do this.

It is difficult to refuse to compare oneself to one's own group. Therefore the shifts in level of aspiration resulting from comparison with the college group are similar for the like and the expect category.

When placed above graduate students the like group consistently shift their level of aspiration downward to a very great degree, but when placed below graduate students they also shift downward. The former is in line with the theory but no ready explanation of the latter finding is available. Perhaps the position below a group of high status forces them to become more realistic in their behavior.

Our last conclusion (4), derived from the greater fluidity of the irreality level, is related to the amount of shift in the like as compared to the expect group. Our hypothesis is that the like group more easily than the expect group avoid failure by refusing to accept group standards and more easily create for themselves a feeling of success by accepting group standards. This is substantiated by the fact that for the like group the shifts occurring on being placed above a group are of much greater magnitude than those occurring when they are placed below a group. The magnitudes are very nearly the same in the case of the expect group.

ACCEPTANCE OF GROUP STANDARDS AND FEELINGS OF SUCCESS AND FAILURE

Our theory states that the acceptance of group standards depends upon whether such acceptance means success or failure. If this is correct, then we should expect that with acceptance of a 'favorable group standard' the general feeling of being 'successful' should increase. With the acceptance of an 'unfavorable' group comparison the general feeling of being unsuccessful should increase.

As previously reported (3) the interviews held with the subjects after the experiment were rated according to the degree of the subject's feeling of success and failure in each session as a whole. The analysis (3, Table V) shows that in every case where the discrepancy score shifted downward a change in the success-failure ratings occurred. These changes indicate more feeling of success or less feeling of failure, both of which imply the same direction of change. In every case of an upward shift in discrepancy score the opposite direction is indicated in the success-failure ratings. This relation holds for all groups including the below graduate student group. These results strongly support our theory.

THE EFFECT OF THE RELATIVE WEIGHT OF THE GROUP STANDARDS ON SHIFT IN DISCREPANCY SCORE

Gould and Lewin (4) show that a variety of problems related to level of aspiration can be dealt with by distinguishing different frames of reference existing simultaneously for the person. Each of these frames of reference contributes a specific amount of positive or negative valence to a certain difficulty level. One of these frames of reference is one's own past performance which gives to higher achievements a positive valence and to lower achievements usually a negative valence.

One might regard the introduction of the comparison group scores into the life space of the subject as the introduction of a new frame of reference which will in part determine the positions of our hypothetical curves. One can assume that

the only frame of reference which existed previously was the individual's own performance. The extent to which the individual accepts this new frame of reference should determine the extent to which his level of aspiration will shift in the direction expected from our theory.³

When the individual is told what others have scored, he is placed in an overlapping situation. He has two sets of standards before him: his own, which up to this time he has been using, and those of the group. The more potent (relative weight) the group standards are, the greater should be the magnitude of the shifts.

The construct 'potency' is conceptually relatively simple (6), but much difficulty has been encountered in its operational definition. One successful attempt at such a definition has been made by Barker, Dembo and Lewin (1) in determining potency of a background of frustration for an immediate situation.

As reported in our previous paper (3) the interviews were rated on the amount of attention which the individual paid to the standards of the group to which he was compared. This rating was done on a ten-point scale and forms a convenient operational definition of the potency of that frame of reference relative to the other frames of reference which determine the level of aspiration.

To get reliable means the subjects were arranged in three groups: those for whom the group had a potency of 1, 2, or 3, those for whom the potency of the group was 4, 5, or 6, and those for whom these values were 7, 8, or 9. In no case was the potency rated as 10.

The curves of the potency of the group plotted against the amount of change in discrepancy score are presented in Fig. 3. The greater the potency of the group standards as a frame of reference, the more does the discrepancy score change from its previous position. This holds for both like and expect groups.

³ We have seen that the like group was able to reject completely this new frame of reference when they were placed below high school students.

The curve for the like group consistently lies above the curve of the expect group. This is additional proof of the greater fluidity of the more irreal behavior of the like group.

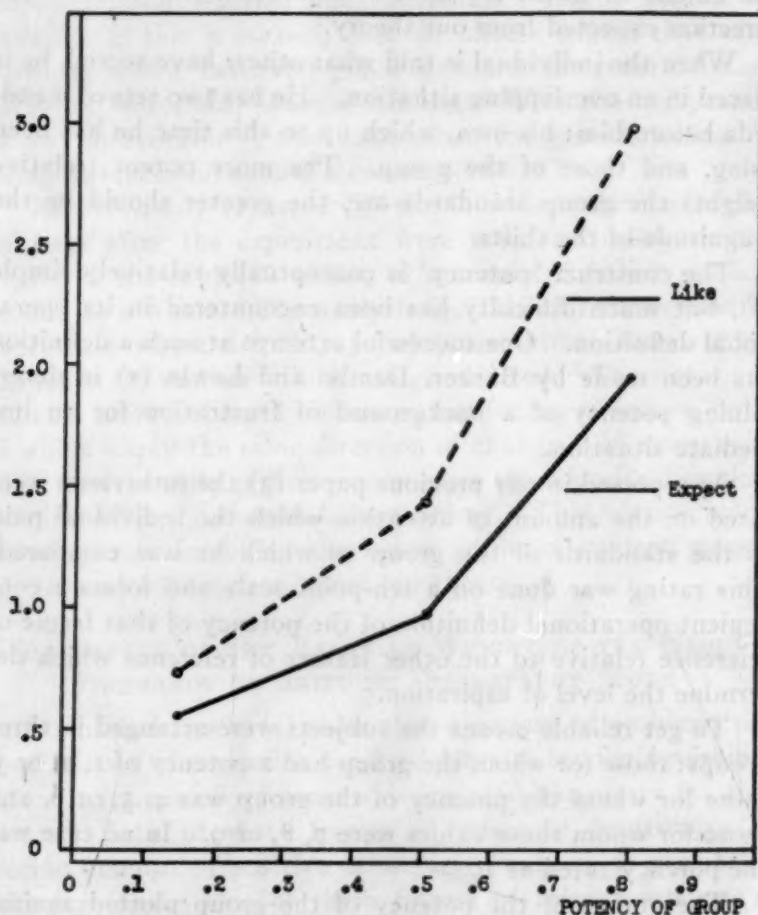


FIG. 3. Magnitude of change in discrepancy score as a function of the potency of the comparison group.

SUMMARY

This paper deals with the theoretical questions concerning the effect which the knowledge of group standards has on the level of aspiration of the individual. It also deals with the

difference between wishes (like group) and expectations (expect group) in this respect. The experimental data on which our theory is based have been summarized. These data consist of responses of college students who were placed in positions above and below high school, college, and graduate student groups.

The experimental results become understandable if one analyzes (a) the strength and direction of the driving forces, (b) the strength of the restraining forces and (c) the potency (relative weight) of certain frames of reference.

The following derivations were made and are corroborated by the experimental data:

(a) The direction of shift in discrepancy was derived from the changes in valence resulting from the introduction of a new frame of reference.

(b) The magnitude of shift was greater, the greater the potency of the new frame of reference.

(c) The magnitude of shift was greater, the smaller the restraining forces. This means that the discrepancy score is greater for the like group than for the expect group, and (2) that the shifts in discrepancy score are greater for the like than for the expect group.

(d) The relative weakness of restraining forces on the level of irreality enables easier structuring according to one's wishes. It follows from this that: (1) in accepting or refusing to accept the standards of a group as a frame of reference for one's own level of aspiration, the like group is guided more by their wishes than the expect group. The like group refuses acceptance if acceptance would mean failure and accepts if acceptance means success; (2) this differential acceptance can be applied less to the standards of one's own group than to groups above or below one's own status.

It was possible to provide an operational definition of the potency of a frame of reference.

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THE PROBLEM OF MULTIPLE PSYCHOLOGICAL LANGUAGES

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I. INTRODUCTION

One encounters quite often among psychologists committed to a unifocal point of view, resistance to alternative or competing views. This criticism is grounded in such factors as ignorance of the rich developments in scientific methodology of the last 50 years, naïveté as to the rôle of language and language calculi in scientific inquiry, an anti-mathematical bias plus an unshakeable conviction that the mathematical rigour of the physical sciences can and should have no place in psychology, and a desire for easily understood explanations even in those situations where a simple explanation is hardly to be expected. The writer has observed these responses quite often, made with respect to Rashevsky's mathematical neuropsychology (7). The worst barrier seems to be the lack of understanding of the role and the legitimacy of multiple languages for the same phenomenon or for concomitant phenomena upon different aspects of which psychologists of one school or another have focused attention. No single paper could treat this problem of multiple languages adequately but its importance for psychology is sufficient to warrant examining the most general considerations.¹

II. LANGUAGES AND OBSERVABILITY

Definition of a language.—A language is a set of words and/or symbols plus (1), rules for the formation of statements with these words and/or symbols (the so-called rules of syn-

¹ The material worked out here is chiefly the author's own with a substantial debt due those positivists whose concern with the role of language in methodology has influenced the subject beyond measure (2, 3, 6).

tax) and (2), rules for the derivation of consequences (rules of operation). Statements may be described, defined or classified as equipollent, syntactical, semantic, pragmatic, etc. These purely logistic considerations, though important, cannot be discussed here. This omission, however, will not affect the general nature of this discussion (1).

Kinds of languages.—Languages are of two kinds: formal languages, without material content but potentially applicable to empirical phenomena, like mathematics and logical calculi, and material, *i.e.*, empirical languages which are concerned with some portion, though possibly all, of the universe. The concepts in the first have each other for designata, the concepts in the second apply to entities other than themselves and are assumed to be isomorphic with them. The languages of psychology must be empirical, *i.e.*, they must refer to events which are communicable and observable in some sense. This does not mean that formal languages may not be taken over bodily by psychology. It does mean, however, that the signs of such languages will then have as referents, material entities, classes, relations and processes, of a psychological nature.

Empirical languages.—Every empirical language is isomorphic with, *i.e.*, corresponds to, the structure² of that portion and that aspect of the universe to which it is applied. Consequently the concepts denote some aspect of things or events in some portion of the universe, and the structure or property-manifold of the relations posited for the symbols (terms) of the language is also supposed, in some manner, to obtain among the things or events themselves. Empirical languages are of three kinds: (a) strictly non-observable, (b) partly non-observable, and (c) observable.

(a) *Strictly non-observable.*—A language is said to be strictly non-observable when the entities it uses for explanation at some given time are not directly observable and their properties and relations not directly measurable. In principle these may become observable and measurable at some future

²The concept of 'language-event' isomorphism is widely discussed in the literature of the *philosophy of science* and we will here assume the reader's familiarity with it.

time. Thus, for example, before photographs of electron tracks became a reality, *atomistic* entities like molecules, atoms and electrons, and properties such as valence, were features of a strictly non-observable system. It was possible, even at that time, to reason within this system and predict, or extrapolate to, macroscopic events, thus satisfying the fundamental criterion of predictability.

(b) *Partly non-observable*.—A system is said to be partly non-observable when either the entities are directly observable or their properties and relations directly measurable, but not both. Rashevsky's neurological psychology is, in one respect, a partly non-observable system. Neurons are directly observable microscopically but their postulated properties, such as the possession of circuits of a given sort and the postulated relations among any number of such circuits, are strictly non-observable *at present*.

(c) *Observable systems*.—These are systems in which both the entities and posited relations may be observed. Systematic botany is one such system, gross anatomy another. Many of the social sciences such as anthropology are, to a substantial extent, of this kind.

Most scientific systems (languages) are usually mixtures of observable and non-observable elements. With respect to non-observability a distinction should be kept in mind between *non-observability in principle* and *non-observability in practice*. The former refers to an observation which is impossible in a given system by virtue of physical laws already part of the system or by virtue of the definitions or the logic of the system. Thus the Principle of Indeterminacy in physics makes the *simultaneous observation* of the position and velocity of an electron non-observable in principle by virtue of physical considerations. *Non-observability in practice* simply means that the observation has not been made to date, due usually to instrumental limitations, but may be reasonably expected when these are overcome.

Any language, regardless of whether it falls under (a), (b), or (c), is a scientific language if, in employing concepts with observable referents, it succeeds in making predictions about

macroscopically observable events, as do languages of (b) and (c), or if it succeeds in making these predictions inferentially, as do those of (a). Strictly non-observable languages succeed in doing this after statements in them have been translated into some macroscopic language. We shall have more to say about *translation* later.

Any theoretical neurological psychology of which Rassevsky's is but one example must, by its nature, be a language dealing with partly non-observable events. These are the assumed relations and interactions among neuron assemblages and neuron circuits. These are *non-observable in practice only*, for they may become verifiable relations at some future date by roentgenographic, electroencephalographic, or microchemical means, or by some combination of these. In the meantime such a language has legitimate scientific status if sentences in it may be translated into some macroscopic language in which they become predictions which are subsequently verified. This will be true even if those same predictions can be arrived at in the macroscopic language itself without the help of a partly non-observable language. In that case the two languages (systems of concepts and relations) serve as a check upon one another. Ideally scientists will tend to resort to languages of (a) and (b) only when either (1), prediction can be arrived at not obtainable from some macroscopic language, itself, or (2), more predictions can be arrived at concerning the level of inquiry with which a macroscopic language is concerned than can be obtained by the macroscopic language, itself, or (3), when types (a) or (b) on the one hand, or (c) on the other yield the same data, but types (a) or (b) do so more easily than any macroscopic language.

III. THE THEORY OF CHOICE IN NON-OBSERVABLE SYSTEMS

In order to explain a given class of macroscopic phenomena, theoretically there may be an infinite number of *sets* of non-observable entities which we may postulate, and for each of these sets theoretically we may posit an infinite number of

relations. On the other hand there is also an infinite number of relations which cannot be posited for each set of entities. The latter will consist of those relations which are physically or logically self-contradictory, or of any relation that implies the incorrectness of some established fact. In this discussion, however, we are not concerned with criteria for detecting unallowable entities or relations but only with the means for choosing among the allowable sets of these. Let us suppose then, that a group of inquirers arbitrarily agrees to use some postulated set of non-observable entities. Since there is no criterion of choice the agreement will be consensual. However, if we designate the various sets of relations (assumed finite for the sake of discussion) which may be posited for any given set of entities as S_1, S_2, \dots, S_n , then there are two principles of selection to guide our choice. (1) The simplest set is the most desirable. (2) The set which yields the largest number of logical consequences with empirical meaning (as in the case of net effects in atomistic explanations) when translated into some macroscopic language is the most desirable. By applying (2) first we find, let us say, that S_5, S_{11} , and S_{15} , each gives a maximum number of consequences with empirical meaning. Now defining (1) in some conventional way, such as that the number of relations posited shall be a minimum or shall satisfy the criteria ordinarily used in postulate systems, we may select S_5 as the simplest of these. Again we might take the quotient of the number of empirical consequences predicted and obtained, divided by the number of relations posited, and choose the set for which this value is highest. This, in a sense, would constitute an *index of system efficiency*. In this latter case the system of relations might not turn out to be the simplest in the mere numerical sense, for among all the systems of posited relations at our disposal, each of which satisfies conventional postulate-system criteria, that one which gives the largest value for the quotient might have the largest number of posited relations. In general, we can define 'simplest' by reference to *relations* alone or *consequences* alone or, better still, to the combination of the two. If after we did this

there was still more than one such set available we could weight consequences³ for their importance by a consensus judgment and narrow down the possible choices. If now there were still more than one set these would all be of equal value by definition. We would, however, never weight the relations, since the quotients themselves reflect the significance of posited relations.

Up to this point we have assumed that the system of entities was chosen arbitrarily. We could, however, now carry this procedure one step further and apply the several sets of relations, $S_1, S_2, \dots S_n$, to each of the systems of entities, $E_1, E_2, \dots E_n$, in turn. Each application of the finite number of S-systems to each E-system would yield a final choice. We would contrast these final choices and that combination of an E-system with an S-system yielding the largest *index of system efficiency* would constitute the *best choice* of a non-observable system for the given class of macroscopic phenomena. We anticipate that this *best choice* will continue to be *best*. In practice, observing the interplay of experiment and changing theory, we note the following. First, the choice that is best varies from time to time, depending upon the state of our knowledge, though no choice is ever the *best choice* more than once. Second, our choice is not deliberate in the manner of the above description but is actually hit and miss, so that both our explanatory entities and the relations posited for them may vary as time goes on. As long, however, as the E-system's components are strictly non-observable in practice and its S-system not directly verifiable, even though particular E-components and/or S-components may change, we are talking in a language that makes use of a non-observable system, and its status will depend, as will the

³ If we designate an entity-system as an E-system and a relations-system as an S-system, then the consequences of the systems under discussion here are assumed to be compatible. However, incompatible consequences, themselves, tell us that the E-system or the S-system which we are employing is in need of modification. An E-system or an S-system will also be modified when a prediction is not obtained. The purpose of such modification will be to obtain a system which will be incapable of yielding the prediction obtained previously. Modification will also be necessary when a phenomenon actually occurs which should not be possible according to the non-observable system being used.

language of a partly observable or wholly observable system, on its ability to predict and to explain.

Anything which is true of observable and mixed systems with empirical predictability will be true of strictly non-observable systems with empirical predictability. The procedure of choice will, however, be simpler for the former two types although there is no essential difference among the three so long as empirically verifiable predictions can be obtained from any type of language system. The most popular system in the long run will be the one that has been most fruitful, and may be either non-observable, observable, or mixed. A given class of macroscopic phenomena may be explained by one system at one time and another at another, but whatever the system employed, it is a language and it is a language concerned with empirical predictability and explanation.

IV. THE NATURE OF EVENTS

There are two kinds of events in nature classified with respect to time. These are concomitant events and non-concomitant events. Two events are said to be concomitant when the time interval between them is exactly zero, either by measurement or assumption. Two events are said to be non-concomitant when the time interval between them is finite, either by measurement or by assumption. Each of these can be divided in turn. Concomitant events can be divided into causally concomitant or concurrently concomitant. Two events are said to be causally concomitant when they have the same immediate or remote cause or when their respective causes are functions of one another, either by measurement or by assumption. Thus a given stimulus is the immediate cause both of an event that can be described strictly in *S-R* schema as well as of an event that can be described in neural terms, and the neural event together with the behavioral event constitute a pair of concomitant events with the observed stimulus as their proved or assumed immediate cause. Two events are said to be concurrently concomitant when the difference of their starting times, $t_1 - t_2$, equals zero but they do not possess a common cause and their respective

causes are not functions of one another, as determined either by observation or by assumption.

Non-concomitant events may be divided into causally non-concomitant events and independent non-concomitant events. Two events are said to be causally non-concomitant when they have a finite time interval between them, by measurement or by assumption, and are related as cause and effect. Thus the cause of a pair of concomitant events and either member of the pair, itself, if separated by a finite time interval, are causally non-concomitant events. Two events are said to be independent non-concomitant events when they are neither observed nor assumed to be causally connected, although separated in time, and when their respective causes are not functions of one another.

Reducibility and translatability.—Concepts are said to be reducible when *either* one of them means exactly what the other means *or* when a concept can be shown to be equivalent logically to some combination of two or more other concepts. This definition of reducibility holds whether the concepts are in the same or different languages. Such reduction is called constitutive reducibility. There are, of course, other kinds of reducibility but because we find constitutive reducibility primarily in psychological languages, we shall concern ourselves only with this type.⁴

Two sentences in different languages are said to be translatable when they refer to the *same event* or when they refer to causally concomitant events. The rules for translation⁵ between any two languages have to be specified and freed of all ambiguity. We shall not enter into a discussion of such rules here.

⁴ 'Emergent' concepts occur in psychological languages but these require special treatment for reducibility, since they refer to non-summative properties. 'Molar behavior' in phenomenological language, regarded as not derivable in neurological language from summing neuronal circuit matrices, requires additional concepts for description at the neural level, not reducible in a constitutive sense, to those already employed at this level. These additional non-constitutive concepts may then be translatable into the 'referent' at the phenomenological level. This is still reducibility, since there is a one to one correspondence between neural matrices and given 'emergent' properties, but it is not *constitutive reducibility*.

⁵ The reader is assumed to be familiar with the material on this subject in the writings of the logical positivists.

We can now turn to a classification of the kinds of sentence-pair relationships which can be obtained in scientific inquiry by considering the event in relation to the sentence-pair.

I. Two sentences in different languages refer to the same event but some or all of the respective concepts are equivalent. These are *substantially equivalent* and *conceptually equivalent in whole or in part*.

II. Two sentences refer to the same event but all of the concepts in one have meanings different from any of those in the other. These are *substantially equivalent* and *conceptually different in entirety*.

III. Two sentences refer to two causally concomitant events but some or all of the respective concepts are equivalent. These are *substantially different* but *conceptually equivalent in whole or in part*.

IV. Two sentences refer to two causally concomitant events but all of the concepts in one have meanings different from any of those in the other. These are *substantially different* and *conceptually different in entirety*.

V. Two sentences refer to two causally non-concomitant events but some or all of the respective concepts are equivalent. These are *substantially different* but *intensionally equivalent in whole or in part*.⁶

VI. Two sentences refer to two causally non-concomitant events but all of the concepts in one have meanings different from any of those in the other. These are *substantially different* and *intensionally different in entirety*.

These are the chief types of events with which we are concerned in psychology.

From the foregoing it can be seen that reducibility may sometimes accompany translatability as in sentences of Type I. Between the sentence-pairs of many psychological languages concepts will be found to possess constitutive reducibility, although in general reducibility and translatability are independent operations. Without digressing into a tech-

⁶ We use the phrase *intensionally equivalent* to distinguish V and VI from I, II, III and IV.

nical discussion of translatability let us mention that translatability becomes effective in those situations in which only syntactical operations are necessary to convert one sentence into another, or when we have partial or complete reducibility as in sentences of Type I, or when redefinition of one sentence subsumes the meaning of the other. Most important of all to us as psychologists is the translatability which may occur when two causally concomitant events are related in a one to one correspondence, so that although they each refer to *substantially different events*, we state that if one occurs the other does also. In this case the assertion of one implies the empirical existence of the other and this kind of connectedness permits a common variety of translatability. This connectedness, however, is not a necessary connectedness either in a logical or a physical sense, since the event corresponding to one of the two sentences may be only an assumption, *i.e.*, entirely or partially non-observable, as with neural events. Between two branches of mathematics, as with rectangular and polar coordinates, and between mathematics and a sentence from a natural or social science, as when the value for a slope has a physical meaning or a social meaning, we have a common type of translatability through isomorphism. Probability provides another simple example as when the normal probability function is increasingly confirmed at the limit, empirically, by tossing coins a great number of times, even though the normal curve may be derived without reference to any empirical entities whatsoever by the binomial expansion. A translated sentence, it must be remembered, is not a transformed one, except with sentences of Type I.

Language-pair relationships.—Let us denote language one by L_1 , and language two by L_2 . Let us further agree to let the symbol, sL_1 , mean 'some of the sentences in L_1 ,' the symbol, aL_1 , mean 'all of the sentences in L_1 ' and the symbol, nL_1 , 'none of the sentences in L_1 .' Then, between any two languages, the following symmetrical and asymmetrical formal relations of translatability are possible, of which one may be

true of a given pair at one time and false at another. Using the symbol ' $<$ ' for 'are included in' we have:⁷

- | | |
|-----------------------------------|-----------------------------------|
| (1) $sL_1 < L_2$ and $sL_2 < L_1$ | (4) $sL_1 < L_2$ and $nL_2 < L_1$ |
| (2) $aL_1 < L_2$ and $sL_2 < L_1$ | (5) $aL_1 < L_2$ and $nL_2 < L_1$ |
| (3) $nL_1 < L_2$ and $sL_2 < L_1$ | (6) $nL_1 < L_2$ and $nL_2 < L_1$ |
| (7) $sL_1 < L_2$ and $aL_2 < L_1$ | |
| (8) $aL_1 < L_2$ and $aL_2 < L_1$ | |
| (9) $nL_1 < L_2$ and $aL_2 < L_1$ | |

This gives us six and only six possible language-pair relationships. Some of the existing languages for which these hold will be mentioned shortly. Here we might briefly point out that if L_1 is a mixed language which includes, among other things, assumed neural events, such as Rashevsky's, and L_2 is a pure phenomenological behaviorism, that is a language system of completely observable S-R connections and schema, then number (3) would apply if there were *at least one sentence* of L_2 translatable into L_1 . This is so because, by virtue of its nature, no sentence in L_2 can express a sentence in L_1 , since *all neural events*, assumed or observed, are excluded from L_2 forever.

Any total field of inquiry, F , such as human behavior, can be divided into several parts, such that each part may be made to contain a class of homogeneous phenomena, where the homogeneity is established by definition, by some useful set of criteria, or by some kind of arbitrary demarcation. Each of these subdivisions can be ordered to give a series P_1, P_2, \dots, P_n . A series of languages may then develop for inquiry into each of these areas and likewise ordered into a one-to-one correspondence with them. If the field F were diagrammed there would, of course, be no overlap between any two P 's. If the languages were diagrammed on the basis

⁷ Nos. (3) and (4), (2) and (7), (5) and (9), respectively, are identical pairs with the language subscripts interchanged. (6) is a condition of non-translatability or mutual exclusiveness. (8) is the condition for complete translatability. This condition will occur when (a) two languages have been developed for the same set of events or (b), two languages have been developed for two sets of causally concomitant events such that there is perfect one to one correspondence between them, i.e., for every event in one there occurs *always* an event in the other.

of their mutual translatabilities, a complicated diagram would result in which much overlapping would occur, the nature of the overlap being describable for any pair in terms of one of the six possible *language-pair* relationships spoken of above, and any one language might be wholly or in part translatable into several others.

V. LANGUAGE SYSTEMS AND THEIR RELATIONSHIPS

Psychologists are concerned with behavior, human and animal, individual or group. However, when a single human being reacts to another many concomitant events are occurring, depending upon the observer's particular field of inquiry, or *P*. Neural events, events describable only in *S-R* schema, physiological events, sociological events, anthropological events, all these and many others are occurring simultaneously. Keeping in mind the aforementioned *P*-series and *L*-series, let us designate a system in which many concomitant events are occurring as *F*, the total field of inquiry. Let each class of phenomena in *F*, homogeneous by definition or demarcation, be designated as $P_1, P_2, P_3 \dots P_n$. Let the language systems (concepts plus relations) isomorphic to these classes be called $L_1, L_2, L_3 \dots L_n$. Selecting any two languages at random, say L_1 and L_2 , let us examine some of their possible relations and properties.

1. L_1 and L_2 separately, after the syntactical rules of formation and transformation have been established for each, may be able to yield logical consequences capable of experimental verification for P_1 and P_2 respectively. These are scientific virtues even without translatability.

2. Some phenomena, other than P_1 -phenomena or P_2 -phenomena, may be capable of prediction in L_1 after sentences in the language corresponding to the class of phenomena in question have been translated into L_1 and made to yield consequences in L_1 . This may not be true of L_2 .

3. L_1 may not only be able to predict certain P_2 -phenomena qualitatively but may also be able to predict some P_2 -phenomena *quantitatively*, by means of a mathematical rationale which L_2 , itself, does not possess. Certain physical

constants may be predictable in L_1 but not in L_2 . This appears to be a possible relationship, as time goes on, between Rashevsky's system, L_1 , and that of a phenomenological behaviorism, L_2 .

4. If we regard L_N as the language of Rashevsky's neuro-psychology and L_B as the language of a strict phenomenological behaviorism, that is to say, one which wishes to concern itself only with the temporal relations of S - R schema, then L_N is a mixed system of (1), observable elements (neurones, data with respect to nerve fibers, chemical cell properties) and (2), assumed non-observables (specific types of neural circuits, hysteresis circuits, excitatory and inhibitory substances and fibers, etc.). L_N is concerned with predicting net effects of relations posited for L_N elements, as P_N -events (neurological phenomena) and the possibility is therefore present, even where no verification is as yet possible for P_N -phenomena as predicted, of translating these prognostic L_N statements into L_B statements. Likewise, if there is a language of physical chemistry, L_C , and of electro-chemistry of nerve conduction, L_E , there is the possibility of translating L_N statements into L_C or L_E . This will occur when L_N makes statements about geometrical configurations of neuro-elements and certain relations between them while utilizing a minimum of L_C or L_E assumptions as part of the system L_N .

5. L_N , by virtue of its being a mixed system, is flexible and can absorb any new conceptual elements with which its achieved conceptual structure is not incompatible. L_B , by definition, is limited not only to observable concept correspondences, but just two kinds of these, stimuli and responses, whose time relations in various combinations give L_B laws or regularities. For this reason, if there are any classes of phenomena in F not describable or predictable by L_B concepts, they may be predictable and describable by some other language, possibly L_N , but any phenomena not capable of handling by L_B , as, for instance P_N -phenomena, must, by definition, be ruled out of P_B altogether, a defect not suffered by a more elastic language whose corresponding phenomena are either causally concomitant with P_B -phenomena or overlap

or include P_B -phenomena, as well as the original phenomena for which that language was intended.

6. The charge might be made that L_N to date had never been able to make a single prediction. Even if this charge were true, there is still the possibility that L_N might sooner or later reach a stage of development, without negating its present position, in which predictions may be made at the neural level itself, translatable into some macroscopic language. Suppose it had also been asserted that L_N , in its present stage, was not translatable in any way into L_B . We might counter this by saying that here again the possibility exists that *later* an L_N statement may be translatable into L_B as well as an L_N prediction into L_B . If L_N *was capable* of predictions but as yet there were no phenomena recognizable as causally concomitant to these L_N predictions, then L_N would be quasi-formal, that is to say, its predictions would not have translatability, at the moment, into another empirical language, but might, *in principle*, at some future time be verified as L_N -events, as described under point eight, below. Failure to find an empirical language, at present, into which L_N statements are translatable does not preclude our finding one in the future, unless it can, indeed, be shown that L_N is only a formal language. This is impossible, by virtue of its distinguishing, observable neuroelements. None of the foregoing, however, are charges which can justly be brought against Rashevsky.

7. A language like L_P , the language of psychoanalysis, may be able to show that some of its distinguishing concepts, such as frustration, sublimation, etc., are either L_B concepts with a new name or are syntheses of certain L_B concepts. We then have a case of simple substitutive or constitutive reduction, respectively. However, if there are in L_P , distinguishing concepts, not in L_B through substitution or constitutive reduction, but such that no predictions can be made by them alone or no predictions can be made by them in conjunction with L_B concepts, not already capable of prediction in L_B , then L_P may be said to have neither formal predictability nor empirical predictability and consequently

resembles neither L_B nor L_N . To prove either the formal or empirical status of L_P its exponents would have to state clearly and concisely either its distinguishing rules of syntax or its distinguishing rules of translatability other than translatability by simple substitution or constitutive reduction. On the other hand to prove that L_P is unlike L_B or L_N devolves upon those who attack L_P , but even if such proof were never rigorously given, some of the aforementioned conceptual defects are possessed by L_P as can be seen by casual reading of psychoanalytic literature; for example, the concepts of id, libido and censor.

8. If L_F is the language of F (and there must be such a language of which the sub-languages L_B , L_N , etc. are special cases) it may be that through subsequent growth and flexibility one of the sub-languages may become L_F . By virtue, however, of its own self-imposed restrictions L_B , as well as certain other sub-languages, can never become L_F . L_N , however, has the following virtues. It is a reasonable assumption that there are P_N -events. It may become experimentally possible to observe P_N -events through *instrumental* inference (roentgenography, electroencephalography, etc.). Thus there would be the possibility of verifying L_N statements without reference to L_N translatability. In addition, however, L_N may conceptually grow so as to predict and describe P_B -events, or even, for that matter, events in any part of F . This may on the contrary be true for some other sub-language. This ability, however, to become L_F is forever impossible for L_B which is purely phenomenological, although the universe of discourse of L_B may be arbitrarily defined as psychology proper by its exponents.

9. There is always the possibility that there are phenomena in F not capable of being explained or predicted by L_B or L_N , either at present or at any time. Call such phenomena P_X . Then it follows that there must be some language, L_X , for P_X . It is also possible that L_B and L_N are included in L_X . Suppose this to be so. Now there may be other phenomena in F , P_Y , for which L_X is inadequate but for which L_Y serves. It may also be that L_B , L_N , and L_X are included in L_Y . This

serial regression can continue only as far as L_F . If we believe that linguistic unification is always possible and that any universe is eventually susceptible to explanation in one mode of discourse then we must assume the possibility of L_F for F . Since F and Non- F constitute the universe of all possible inquiry of which psychological inquiry is but a part thereof, ($F \cdot F' < 1$), there must be a language, L_U , the language of this universe, which is not L_F . For the logical positivists this L_U is the physicalistic language as far as translatability is concerned. If we call a statement translated from L_1 into L_2 an *extrinsic explanation* of a P_1 -event and the L_1 statement itself an *intrinsic explanation* of the P_1 -event it may be that L_U will only have, when it is developed, extrinsic explanatory properties. This is to say that, *perhaps*, no statements in L_U will become empirically meaningful, because of its *attenuated extension*, until translated into some sub-language, *i.e.*, until they are converted into, say, statements of P_1 -events in L_1 language. Arguments about this probability are not to the point here. More important for us as psychologists, as well as students of scientific methodology, is that the serial regression with its subsumptive relationships described above, implies a hierarchy of languages (see below), *i.e.*, implies that languages can be ordered with respect to one another, as well as to a P -series. Any L in this order may be taken as a point of reference and the distance of some other language from the reference language should be proportional to the difficulty of translatability, or vice versa. These distances would express position or rank points and not absolute units of *complexity in translatability*. Likewise, theoretically, infinite interpolation of intermediate languages is possible between any two points of the order, such interpolation being determined by the number of different kinds of phenomena empirically or conceptually isolatable between the two points. This is a matter, as we have said before, of definition or demarcation.

10. If a proponent of L_B assumes L_B is L_F (which is contrary to initial definition to start with), at best this implies some redefinition of L_B . If after redefinition, and regardless

of the extent to which F phenomena have also escaped explanation in any one of the other sub-languages of F , there still exists one phenomenon that is not explicable at this juncture in L_B , then the above assumption is still false. The falsity of such an assumption, however, does not imply that L_N explains the given phenomenon or that, even if it does, L_N is L_F .

11. An example of a theoretical difficulty for L_B is the phenomenon of taste. This is not a typical phenomenon for L_B . There is no observable motor response. If we speak of taste as a *response* of the taste bud to food this is really L_C language, or at best, an assumed rather than an observed response. If we redefine *response* in this way than the original L_B position is changed so as to render wider the meaning to be attached to the word 'response,' but a new difficulty is introduced. We must now be able to condition this new type of response to something other than food. If we cannot, we must again either redefine L_B so as to account for this failure, or abandon the new position with respect to the meaning of 'response' by admitting P_B -phenomena are only part of F .

VI. EXAMPLES OF TRANSLATABILITY

At this point some additional clarity may be gained by giving a few examples of pairs of translatable sentences. A brief discussion will then follow. The respective sentence pairs are (1)-(2), (3)-(4), and (5)-(6).

(1)—from neuropsychology: If two internuncial neural matrices, A and B (previously defined), differ in extent and if the number of impulses transmitted by each of them along efferent paths is determined both at the time of initial stimulation and at a later time when a maximum number of impulses appears to have been reached for the original stimuli, respectively, the percentage increase of this maximum over the initial impulses transmitted will be greater for the matrix of greater area.

(2)—from behaviorism: If two tasks (stimulus patterns) differ in some unit of complexity (previously defined) and if the amount of learning for the two tasks is determined initially

from the arbitrary 'zero' origin at the commencement of learning and later at the 'physiological limit,' the percentage increase of the learning at the 'physiological limit' over the initial learning will be greater for the more difficult task.

(3)—from behaviorism: If a second order response is defined as a response to other responses acting as stimuli, then the former is experimentally distinguishable from the latter, cannot be directly localized for any sensory modality, and has the effect of reducing the intensity of stimulation (stimulus values) of the first order responses for us.

(4)—from existentialism: If during introspection we observe our responses to varying stimuli, we shall find that the act of observation can be distinguished analytically from the state observed, that it will not be easy to attribute the act of such observation to stimulation from some particular sensory source, and that the process of observation serves to dissipate gradually the quality and immediacy of our responses, as when, for example, we try to analyze our loves, fears, or hopes.

(5)—from behaviorism: If an organism, through constant reinforcement, finds that a stimulus is pleasant (undefined anthropomorphic assumption), then on the basis of a theory of 'adience' it will seek more of that stimulus and if an interfering stimulus is periodically present which prevents full reception or exposure to the *desired* stimulus, the latter stimulus will be associated with some form of a withdrawal response.

(6)—from psychoanalysis: If a child through frequent association, forms an attachment to the mother and is subsequently forced to forego his selfish desire to possess the mother solely and completely by resigning himself to the recognition that the father will expect and receive a due part of the mother's affection and attention, he will first learn to resent and later hate the father. This we define as the Oedipus complex.

Sentences (1) and (2) are substantially and conceptually different but inasmuch as they refer to concomitant events (one of which is assumed, of course) they are translatable into each other. In this case the suppressed principle of transla-

tion for such a one-to-one correspondence is the following: "If neural matrices differ in extent they differ in complexity of structure and the more complex their respective structures the more difficult the psychological tasks they subserve." The example itself is taken from Gengerelli (4). Sentences (3) and (4) are examples of sentences which refer to the same event and are therefore substantially equivalent and conceptually different; the same holds for (5) and (6).

VII. THE HIERARCHY OF LANGUAGES

Suppose the several language systems concerned with *F*-phenomena are arranged in a continuum, L_1, L_2, \dots, L_N , so as to satisfy the following conditions: (1) Any two adjacent languages shall possess concepts such that some or all of the concepts in that one of them farthest removed from the zero or reference point of the *L*-continuum may be reduced to two or more concepts in the other, but not vice versa. (2) The corresponding phenomena of these two languages, L_x and L_{x+1} , shall be such that P_{x+1} -phenomena can occur only if P_x -phenomena also occur, but P_x -phenomena may occur without P_{x+1} -phenomena necessarily occurring.⁸ In such a continuum or hierarchy of languages translatability from any one member to any other member, adjacent or not, may or may not occur, since, in general, reducibility and translatability are independent properties, although the former may sometimes imply the latter. We say 'may or may not' since there is no way of ascertaining if a sentence in one language may be translated into that of another except by actually trying to effect such a translation, or stating the rules of translation which obtain between the two. In such a continuum, however, the further apart two languages are the more periphrastic will a translation be if, indeed, one can be effected at all. The first and last members of the hierarchy in one direction from the zero point, namely, L_1 and L_N , may be so remote from each other through the respective contrast be-

⁸ Thus, if neural events, P_x -phenomena, occur, behavioral events, P_{x+1} -phenomena must occur but, on the other hand, P_{x+1} -phenomena may occur without P_x -phenomena occurring, as in the obvious case of behavior in organisms with no known nervous systems or nervous-system analogues, which is the case with certain lower forms of life.

tween P_1 -events and P_N -events, as to rule out the possibility of translation between the two. From L_P , however, it must be possible to derive any member of the hierarchy, since L_P bears a relation to each member of the hierarchy which is quite different from that which any constituent member does to any other. Here too, however, because L_P is extrinsic to the entire L -continuum, translations will be circumlocutory, but the point is that translatability from or to L_P is assured.

Not every member of the hierarchy will have scientific status, that is to say, be in a position to make predictions. Thus, if L_P is the language of psychoanalysis, some of its concepts may be reducible to those of an adjacent member, but if no predictions can be made within the system L_P , itself, then there can be only translatability *at present* between L_P and any other language for which predictability obtains. In this case the concepts of L_P would be either new names for those in another language with scientific status, or meaningless.

Lastly let us note that if L_N and L_B are two members of the hierarchy the more predictions there are which we can make in L_N and which after being translated into L_B have become verified, the more certain can we be of the *reality* of P_N -events, even though these are *non-observable in practice*. If these predictions could not be obtained with L_B at all or could not be obtained with L_B , alone, L_B has likewise gained, for changes in the underlying assumptions of L_B may be made which will now enable it to make these predictions all by itself. This situation must always exist between P_N and P_B since they are related as causally concomitant P_X and P_{X+1} events.

VIII. SUMMARY

The hierarchy of languages suggests that multiple psychological languages may be an asset rather than a liability. Gengerelli (5) has made a start in this direction in which the advantages are obvious. We can have no quarrel with a language system that predicts for its corresponding event system. In addition, since language systems overlap when

corresponding event systems do not, a prediction in one may be verified in another, after it has been translated into that other. Finally, for any given event system, one language may make more predictions than another, or make them more easily. These remarks hold whether the language system is strictly non-observable, observable, or mixed. The mathematical neuropsychology of Rashevsky must be judged not by the standards of a naïve empiricism such as is represented by a purely phenomenological behaviorism, but only by the following three criteria: (1) Does it satisfy the varied requirements of a *language system proper* as to syntax, rules of formation and transformation, etc.? (2) Can it make predictions that are in, or may be translated into, empirical terms? (3) Are any of these predictions immediately verifiable, and if not, is there reason to believe that some of them may become so as time goes on?

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FUNCTIONAL AUTONOMY OF MOTIVES AS AN EXTINCTION PHENOMENON¹

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Professor G. W. Allport (1, 2) has called attention to what he and a number of other psychologists (6, p. 33-34) consider to be an important new aspect of human motivation (1, p. 205-206). He has noted that certain acts seem to provide their own motivation, that their original incentive has disappeared, and they therefore seem to be repeated for their own sake. To account for this fact he has created the emergent principle of functional autonomy to displace those theories that treat motives as permutations or elaborations of basic drives. In fairness to Allport's own judgments of the importance of this principle, it should be critically evaluated in reference to those point of view it would displace (1, p. 203-4). Such an evaluation has been attempted by Bertocci (3) from the hormic standpoint, but so far by no one with the traditional view of the learned elaboration of primary into secondary drives. It is to fill this latter need that the present paper was written.

According to the point of view to be presented here a behavioral sequence involving drive may be broken down into three parts (*cf.* 7): (1) An *instigation*, the identifiable event which precedes and initiates activity. In primary drives the instigation is usually thought of as an organic tension, a disequilibrium, or a 'deficit stimulus' (1, p. 204). Instigation is, however, a much broader term than stimulus and is roughly equivalent to the *effect* of stimuli on the organism. (2) *Instrumental acts* which result from the instigation and which

¹ This paper was completed, except for the final revision, while I was at Yale University. Much of it was inspired by my associates there whose assistance is gratefully acknowledged. I am particularly indebted to Prof. Robert R. Sears who read and criticized the manuscript in its early stages.

eventually lead to the goal. They are said to be instrumental because they are the means by which the goal is achieved. They start as random responses to the primary instigation and become instrumental, properly speaking, only after they attain the goal. (3) A *goal response*, the final instrumental act which, because it attains the goal, puts an end to the striving. The counterpart of the goal response in the environment is the reward. The goal response is usually intimately connected with the reward just as the instigation is usually correlated with the stimulus. But in neither case is the correlation always perfect. (Cf. 12, Chap. 5.)

Breaking up the process of motivation in this way does not mean that the parts are independent or that they may be independently measured. It merely provides convenient labels that will be useful in referring to the different parts of the drive sequence in the discussion that follows.

To theorists like Shaffer (17) this is the fundamental pattern of all primary and secondary drives. Disequilibrium produces activity which ceases when equilibrium is restored by the appropriate goal response. Allport would agree that such a pattern may adequately describe the historical or genetic basis of motivation, and that it would serve to represent the operation of primary drives in the behavior of very young children. But he rejects the idea that any such simple scheme could account for the complicated strivings and purposes of adult behavior.

Since this rubric is accepted by the holders of both points of view as descriptive of primary motivation, the problem is to determine where in the elaboration of primary into secondary drives functional independence appears. Learning may produce changes in any aspect of the motivational sequence. New instigators, new instrumental acts, and new goal responses may be acquired. So far as the new acts and new goal responses are concerned they undoubtedly function independently of their predecessors once they are acquired. Therefore it must be in the instigation that the problem of autonomy arises.

It is here according to Allport that a motivational se-

quence cuts loose from its previous history. Instrumental acts persist when the instigations which set them going apparently no longer exist. Therefore the instrumental acts must be performed for their own sake. They must provide their own instigation. Mechanisms have become drives.² An instigation has arisen out of the performance of an act, a *new* instigation which is unrelated to the instigation which originally started the act.

A simple illustration of this phenomenon is to be found in the example of the ex-sailor (1, p. 196) who continues to want to go to the sea although he is now a successful banker. Going-to-sea is an act which was once instrumental to the satisfaction of the hunger drive. *Now* hunger is satisfied by the more adequate instrumental act of working in a bank. Yet going-to-sea persists despite the lack of the original hunger drive. Therefore the act of going to sea must be supported by some other instigation—namely, one which developed out of the act itself.

The crux of this argument for functional autonomy is that the instrumental act ought to have ceased for some reason or other. In all the examples cited by Allport it is assumed that the act which is persisting should have become extinguished or have disappeared. The sailor ought no longer to want to go to sea because his hunger is satisfied by working in a bank. The workman ought no longer to be careful in his work because carefulness is not rewarded (1, p. 196). The neurotic ought to get over his phobia (1, p. 200) because he knows there is no objective reason for it. Throughout all these examples there is the expectation that the instrumental act should have disappeared. And yet the grounds for the expectation are never carefully analyzed. They are more often assumed *a priori*. As a matter of fact there are many circumstances in which one would *not* expect the rapid disappearance of an instrumental act. But these circumstances can best be considered exceptions to the conditions under which one can legitimately expect the cessation of an instru-

² The phrase is Woodworth's (1, p. 195).

mental act. These three conditions, all of which are implicit in Allport's discussion, are: (1) removal of the instigation, (2) replacement by a more successful instrumental act, and (3) removal of the goal response (or reward resulting from the act).

1. The effect of *removal of instigation* may be readily seen from Skinner's curves of satiation (18, p. 354) obtained from data on rate of bar-pressing plotted against time. As the rats became less hungry or thirsty they pressed the bar to get food less often. The instrumental act of bar-pressing tended to drop out as the instigation of hunger or thirst was removed. It is this condition that is the basis for Allport's expectation that Olson's rats (1, p. 199) ought to have stopped scratching when the collodion was removed from their ears. Since the collodion instigated the scratching its removal ought to make the scratching stop. Such is frequently the case. Mowrer (15) found, for example, that removal of fear of shock by disconnecting the electrodes reduced a galvanic skin response. Zener and McCurdy (22) report that conditioned salivation decreases as satiation is approached. Many cases from daily life could be cited in which removal of an instigation does lead to discontinuance of an act. There are many workmen who stop working when they have earned enough money to remove the instigation to work. But the problem that is of importance here is the analysis of those circumstances which may delay the disappearance of an instrumental act after the original instigation has apparently been removed. If it can be shown that there are good reasons why the act should continue, there will be no need to postulate a force within the act that keeps it going. Some of the possible reasons follow.

(a) *Inadequate criteria for the absence of instigation.* It is easier to state that an instigation has been removed than to prove it. Very frequently removal of a physical stimulus does not guarantee that the instigation resulting from it will also be removed (see [c] below). A common criterion for the absence of an instigation used extensively by Allport (1) is a judgment by the experimenter that the instigation should

be gone. Such judgments are subject to considerable error. According to Anderson (3, p. 206) Bayer has found that chickens will continue eating long after they have been judged satiated. Zener (24) has reported the same phenomenon with dogs. The judgments in these cases are difficult because the instigations are internal and because the subjects are animals and cannot be asked whether they are still hungry. But the problem is just as difficult with humans. Even though they may be asked whether a given instigation still exists for them, clinical experience with rationalization and self-deception (*cf.* 1, p. 172, 178 ff.) shows that merely asking a person to report on his motives is not a sufficiently objective way of arriving at the truth. Consequently an act may appear to be continuing of its own accord simply because the judgment that its instigation has been removed is in error.

(b) Improper identification of the instigation. An instigation may be obviously removed (as when the collodion is taken off the rats' ears) but it may not be the right one or the only one for the instrumental act in question. In either case there will be sufficient instigation left to carry the act in question. Many acts have several sources of motivation. A rat runs a maze to be fed and petted as well, so that when he is no longer hungry he may still run (*cf.* 18, p. 372). In the case of the ex-sailor who retained his love for the sea, going-to-sea may have been instrumental to many instigations besides hunger. He may have come to like the easy companionship of sailors. He may have enjoyed freedom from the social restraints of shore life or from a nagging wife at home. Or in Freudian terms he may have found sexual satisfaction from the rocking of the ship (9, p. 600). Removal of the hunger instigation does not guarantee that all other sources of satisfaction from going-to-sea will likewise be removed. Here and in many similar cases it should not be expected that an instrumental act will stop with removal of a supposed source of instigation, because part or all of the true instigation may remain.

(c) Anticipatory instigations. In cases of expectancy a stimulus may be removed without removing the instigation

it has created. For example, foot-withdrawal may be conditioned in dogs to a buzzer with shock as the unconditioned stimulus (10, p. 60). After the withdrawal response has been set up the shock may be discontinued and yet the response will not soon extinguish. The reason for this is that the instigation is no longer the shock but the expectation of shock. Hence the real instigation has not been removed and disappearance of the instrumental act is not to be expected. Genuine removal of the anticipated shock brought about by holding the dog's paw on the grid produces rapid disappearance of the withdrawal response.

The importance of this illustration is that it is the basis of many instances of apparent functional autonomy. The objective basis for fear, need for economy, or whatever, may be removed but the real instigation, which is the *expectation* of trouble, be left untouched. Many acts are motivated at least in part by the fear of the consequences of discontinuing them. The craftsman who keeps to his habits of neatness after there is no objective need for them is afraid of the failure which might come if he stopped them. Failure need only be a remote possibility in his mind to be sufficient to instigate neatness habits. The possibility may appear so remote as not to constitute a real need for an objective observer. But it is real nevertheless to the person involved. It is just such remote eventualities of disaster that keep some people from flying in spite of all the statistics on the safety of air travel. The real instigation in all these cases is the expectation of punishment—an instigation sufficient to delay disappearance of an avoidance response long after the slightest objective possibility of trouble has been removed.

(d) Conditioned instigators. Removal of an instigation does not guarantee that stimuli conditioned to the instrumental act will not remain in sufficient strength to carry the act for some time. The conditioned stimulus will initiate the conditioned response during extinction for some time after the unconditioned stimulus has been removed. To this extent it is a conditioned instigator. It has the power to produce the instrumental act. The more often the act is per-

formed in a rewarding situation the greater will be the number of stimuli conditioned to it and the stronger will be the connection between the conditioned stimuli and the act (10, p. 145, 156). As the instigation spreads in this manner to a number of conditioned stimuli, removal of the primary instigation (the unconditioned stimulus) is less and less effective in producing immediate cessation of the act (cf. 16). The more strongly conditioned the external stimuli are the longer extinction will be delayed (19). Anderson has called particular attention to this conditioning process in maze learning under the name of externalization of drive. He states: "If this internally aroused drive is satisfied over a long period of time in a relatively constant external situation, then the drive mechanism will become aroused by this external situation" (3, p. 207). That is to say, in the present terminology, the external situation has become conditioned to the instrumental act of maze running and will therefore serve to initiate maze running under changed internal conditions (lack of hunger). Anderson later classifies externalization as a case of functional autonomy (3, p. 222). It would be better to classify both phenomena as cases of secondary elaboration of the drive sequence. Many apparent cases of functional autonomy after removal of the primary drive stimulus are due to the continued operation of conditioned instigators. It should be noted, however, that the act will not continue to function indefinitely without its primary instigation. Its eventual extinction is merely delayed and this delay is in proportion to the strength and compounding (16) of the conditioning.

2. Cessation of an instrumental act is ordinarily to be expected if that act is *replaced* by another one which attains the goal more easily or more successfully. It is this replacement that is the basis for Allport's assertion that the ex-sailor should stop going to sea because he is now satisfying his hunger more successfully by working in a bank, and that a rat should give up an established path through a maze when he learns an easier one (1, p. 199). As a matter of fact it is not well established that replacement of one act with another necessarily leads to extinction of the first unless there has

been actual differential reinforcement. To use the reproductive inhibition paradigm, after the connection $A-B$ has been established, practice of $A-K$ until K occurs rather than B when A is presented, does not mean that the previous connection $A-B$ has been extinguished. It means that the strength of $A-B$ cannot be measured in the usual way because it is obscured by the strength of $A-K$. If B and K are responses that can be made alternatively or successively, that is to say, if they are not incompatible, B may appear along with the prepotent K .

This means in the case of the ex-sailor that the association between the hunger instigation (A) and the act of going to sea (B) is not necessarily weakened by the formation of the new association between (A) and working in a bank (K). The former *may* be weakened. There is some evidence from recent work of Melton and associates (13, 14) that it is weakened under certain conditions of incompatibility. But there is sufficient doubt as yet as to the generality of these findings to make it essential to show that there actually has been differential reinforcement of the two instrumental acts before it can be assumed that the first will be extinguished when replaced by the second more successful act.

3. The final basis for expecting an instrumental act to drop out is the most common of all—*removal of reward* resulting from the act. This is the basis for Allport's assertion that the mollusc which had burrowed in the sand (1, p. 199) because of the tides should stop burrowing when moved to the laboratory where there were no tides, and his assertion that the workmen who had speeded up their rate of work in response to added incentives should slow down again when these rewards were removed. Cessation of an act because of removal of reward is technically extinction (10, p. 344). Extinction in ordinary conditioning situations does not occur rapidly (19). It takes a large number of successive repetitions and it is usually not permanent since it is subject to spontaneous recovery (10, p. 129). *Normally* then, immediate cessation of an act which has been instrumental should not be expected

when reward is removed. But there are a number of factors which may delay extinction even longer.

(a) Distribution of extinction trials. Frequently in real life situations involving extinction the unrewarded instrumental act does not occur frequently enough to bring about the necessary conditions for extinction. If the repetitions are not massed but spaced over intervals of time, spontaneous recovery may occur between repetitions and extinction be more or less permanently postponed (10, p. 133). Such acts as voting for a particular candidate or participating in a war, for example, may not extinguish because they are not repeated often enough without reward.

(b) Random reinforcement. Humphreys (11) has shown that acts rewarded only part of the time during acquisition take a much longer time to extinguish. This fact is particularly important in regard to those human motivational situations with which Allport is dealing because reinforcement in such cases seldom occurs every time. Most fishermen, for example, are rewarded only occasionally for their efforts. It would take therefore a great many successive discouragements to make them stop fishing altogether. Often a person will continue an act long after the possibility of reward appears to others to be completely exhausted because he has been rewarded only occasionally in the past. The workmen may continue to work fast in the hope or expectation of receiving the added reward even after it has been discontinued.

(c) Secondary rewards. Wolfe (21) and Bugelski (5) have shown in two quite different situations that certain stimuli associated with the primary reward may come by learning to have a reward value of their own. In Wolfe's study chimpanzees learned to work for poker chips which could be exchanged for food. If the food reward was taken away, that is to say, if the poker chips were 'worthless,' the animals still continued to work for them for some time. One reason for this was that they could not discover immediately that the chips had no value. Similarly in life situations secondary rewards like paper money, prestige symbols etc. may serve to reinforce certain acts after their objective reward value is lost,

because it is not always possible to assess their exchange value easily. In Bugelski's study the food that rats obtained by pressing a bar was ordinarily accompanied by a click. When the food was taken away and the click left to operate as during training, the response took longer to be extinguished than it did when both food and click were removed. The click had a derived rewarding effect and served to delay extinction of the bar-pressing habit. It follows that removal of the primary reward is not sufficient grounds for expecting extinction of an act unless it can be shown that there are no secondary rewards supporting the act, or that they too have been removed.

(d) Generalization of reinforcement. Williams (20) has shown that reinforcement of one bar-pressing habit strengthens another bar-pressing habit. That is to say, reinforcement on the horizontal bar prolonged extinction on the vertical bar in the Skinner box. It is not known at present along what dimensions reward may generalize, but it is significant that in this case the generalization was from one habit to another habit instrumental in obtaining the same reward. Therefore it is quite possible that reward of an instrumental act like working in a bank might spread to other acts like going to sea which had been instrumental in obtaining the same reward. In other words going to sea in the case of the ex-sailor may actually be maintained by the rewards obtained from working in a bank. To use another illustration, a millionaire may be stingy at home—although the reward he gets for his penny-pinching means nothing to him—because the rewards from stinginess in the business world have generalized to support similar but non-rewarded habits at home. Consequently extinction of an instrumental act with removal of its reward cannot be expected unless it can be shown that it is not being supported by the generalized effect of the reward for another similar instrumental act.³

When the grounds for expecting cessation of an instru-

³ A really adequate discussion of the effects of transfer of reward would lead far afield. The present statement is an attempt merely to state a *possibility* which Allport appears to have overlooked.

mental act are scrutinized carefully in this manner, it appears that there are many conditions under which such an expectation is not justified. There are many factors—some of which have been suggested here—which are known to delay extinction. If it can be shown that because of the operation of any of these factors *an instrumental act should not be expected* to disappear, Allport's main argument for functional autonomy no longer carries conviction. It is no longer necessary to postulate a force within the act to explain why it persists.⁴

In conclusion then, to theorists who regard adult motivation as the learned elaboration of basic drives, functional independence of an instrumental act is a special case of extinction which is delayed longer than the observer expects because there are unusual factors present in the situation which invalidate the normal expectation. Consequently functional autonomy will be regarded by such psychologists as a theoretically unnecessary concept until an experimental instance can be found in which an act continues when the operation of all these delaying factors has been ruled out with reasonable certainty.

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⁴ Perhaps functional autonomy is a term which covers such phenomena as secondary rewards or conditioned instigators. In that case it would seem preferable to treat these phenomena separately in their learning context rather than to create from them a confusing new principle.

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THE CONCEPT OF ADJUSTMENT AND THE PROBLEM OF NORMS

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Certain questions have arisen in the minds of the more thoughtful psychologists concerning the normative aspect of the concept of adjustment. What, exactly, does a low score on an adjustment questionnaire indicate? Is adjustment synonymous with conformity? If so, were such historical characters as Socrates, Jesus, or Joan of Arc adjusted? If they were not, is adjustment desirable? An adequate answer to these and related questions will entail an analysis of the concept of adjustment and certain of its implicit ramifications.

First, it might be well to trace in a cursory manner the history of the term. The term adjustment is, apparently, with a slight change in connotation, the 'adaptation' of the evolutionists, Darwin, Huxley, and Spencer, and more recently of the functional psychologists, Dewey, Angell, and Carr. With the rise of the evolutionary viewpoint in the nineteenth century, emphasizing as it did the 'struggle for existence,' attention was drawn quite naturally in a new way to the relation of the individual to his environment. The various means and modes of adaptation to environment of the entire animal kingdom were discussed by the writers of the period. Of these writers, Herbert Spencer was perhaps the one who stressed most the idea of the adjustment of the individual organism to its surroundings. In fact, he defined life as "the continuous adjustment of internal relations to external relations" (7). For Spencer, each step upward on the evolutionary scale adds to the complexity of these relations which exist between the organism and its environment. At each level, however, he maintained that perfect life consists

in perfect adjustment. The persistence of this adjustment norm is noteworthy in contemporary psychology, and will be referred to later.

The functional school of psychology, which was a direct outgrowth of the writings of the evolutionists, did not add much to Spencer's concept of adjustment. The term, however, is found in various places in the writings of members of this school, and it is largely to these that we owe its use in psychology today. Since Spencer's time, the term has lost its rugged biological emphasis, and has come to have a sophisticated, more specialized connotation. At the present time, its use is confined principally to the fields of personality, mental hygiene, and social psychology.

The term adjustment as it is used today has three important implications. First, any adjustment situation always involves two factors: There is an individual, psychological factor, and there is a social or, more broadly, an environmental factor. The solution of an adjustment problem will involve the alteration of one or both of these factors. Second, the term implies a specific frame of reference. Judgments of a particular individual's adjustment may be made concerning his relationship to his family, his vocation, or any other aspect of his environment. An individual may be considered adjusted in one situation and not in another. It should be noted in this connection that the possible frames of reference for any specific individual are ethnologically quite arbitrary (8, Ch. XXIII). It is conceivable, for instance, that a person who is not satisfactorily adjusted in one culture or social system might be happily adjusted in another. Third, and most important for the purposes of this discussion, the term adjustment is implicitly normative. Any judgment concerning the adjustment of an individual in any frame of reference is always relative to a norm or standard with which it is compared.

Concerning this last point, there are accordingly two fundamentally different ways in which the term adjustment may be used, depending upon the type of norm employed. First, it may be used as a purely descriptive term. When

used in this way, its correct opposite is *non-adjustment*.¹ Second, it may be used as an ethical term to denote desirability or undesirability, approval or disapproval. When employed in this manner, its opposite is *maladjustment*. Let us first consider its use as a descriptive term.

Since the normative problem is the principal concern of this paper, the question may be asked, What norm is to be used as a basis for making judgments concerning descriptive adjustments? When may it be said that a state of descriptive or empirical adjustment exists? This problem may be viewed either from the standpoint of the psychology of personality or that of social psychology. The latter tends to consider it from the point of view of the environmental factor in the adjustment situation, and the psychology of personality from the individual standpoint. Consequently, two different norms, each representing a different emphasis, are found in current literature. From the standpoint of social psychology, the norm usually employed is a certain minimum of conflict between the individual's behavior and the existing social institutions. LaPiere and Farnsworth (3, pp. 313, 314) state it as follows:

It is not . . . upon subjective grounds that the social psychologist terms a person socially maladjusted. . . . That individual is maladjusted whose personality includes modes of behavior . . . which deviate so much in degree and mode from the norm of group behavior that he is socially classified as abnormal and is, therefore, treated in some exceptional manner by society.

Thus it is seen that the problem of a descriptive social norm is amenable to a statistical approach. The unadjusted individual is one who fails to conform to certain institutions. Since an institution is a mode of behavior to which a majority of the individuals in a particular frame of reference conform, unadjusted individuals may be denoted by their deviation from a statistical norm. Of course the degree of deviation required of an individual before he is classed as unadjusted is to some extent arbitrary.

¹ The correct adjectival form would be either *non-adjusted* or *unadjusted*. It is unnecessary to point out that this usage is not always followed.

There are two individual descriptive norms which are widely used by psychologists today. One is essentially biological, the other psychological. The first is stated in characteristic fashion by Shaffer (6, p. 122): "The sole criterion of what constitutes the solution of a problem is *tension reduction*. Any response that reduces the drive-tension and thereby brings the activity sequence to an end is a solution of the adjustment." It is apparent that if this statement is taken at its face value suicide is the obvious panacean 'solution' to all adjustment problems; death is the great tension reducer. Undoubtedly, then, Professor Shaffer is advocating a *living* state of minimum tension as an adjustment norm.

The other norm in common use is the psychological one of integration. Thus an individual is said to be adjusted when he is integrated. There is some difference of opinion regarding the concept of integration. Is, for instance, integration to be considered as a function of simplicity, or is it to be a relative term? Roback (5, p. 558) propounds this dilemma as follows:

Suppose that we speak of a machine as perfectly integrated when its parts are so assembled as to be useful in turning out a certain product. Yet the machine becomes more interesting, invested with greater character, figuratively speaking, if it can be put to more uses than one, let us say by a collapsing device. To be sure, it is impossible to regard such a machine as a *more* highly integrated piece of apparatus, so that we might have a hierarchy of integration. But on the other hand it is possible to hold that such a device renders the machine subject to interference among the parts and therefore make it *less* integrated.

There have been attempts to define integration in physiological, more specifically, neurological terms. There is such a relatively small amount of factual knowledge about any chemical or morphological changes in the nervous system which are correlated with other bodily changes, however, that such attempts have been highly speculative and of little value. In the words of G. W. Allport (1, pp. 140, 141):

That there are physiological correlates of integration . . . no one will deny. But since integration implies *functional* joining of

nervous pathways . . . and since this entire process lies still in the limbo of scientific mystery, all accounts of integrative growth in physiological terms are at present highly speculative.

Recent writers have concurred in the use of a hierarchical scheme of integration with native and conditioned reflexes at the lowest level, and the complete personality as emergent at the highest level. Integration is thus seen as the harmonious coöperation of the various levels of the personality. Young (9, p. 785) defines it as the "coordinated working of the total organism toward the attainment of some end, goal, or purpose." Stagner (8, p. 76) defines it more generally as consistency of response. In the various definitions, however, there are the common factors of lack of conflict, and consistency or unity of the personality. If integration is employed as an adjustment norm, then, the degree to which these conditions exist is an index of the degree of adjustment. If these criteria are met, the individual is adjusted; if they are not, he is unadjusted.

We have thus far considered descriptive adjustment norms, both social and individual. The problem that now confronts us concerns the limitations of such norms. Very simply, descriptive norms are limited by the fact that it is impossible, using them merely as descriptive norms, to say anything about the desirability or undesirability of an individual's adjustment. We can say that an individual is adjusted or that he is unadjusted in the sense that he conforms to certain institutional norms or that he does not, or in the sense that he is integrated or in a state of minimal tension or that he is not. But we cannot say that conformity is desirable or that lack of integration or a high degree of drive-tension, to use Shaffer's term, is undesirable.

Now it may be very well for the scientific psychologist to refuse to consider the problem of the desirability of adjustments on the ground that "Good and bad are essentially ethical concepts and have no place in the realm of science" (6, p. 136). But ethical interests are far more basic than scientific interests. Those persons engaged in the task of attempting to aid in the solution of adjustment problems cannot

remain completely aloof. All application of scientific knowledge and techniques is relative to the realization of certain values. We may ignore the problem but we cannot escape it. In a paper such as this, no attempt can be made to deal in any adequate way with this problem. Since, however, descriptive norms are sometimes used, either explicitly or implicitly, as ethical norms, it might be of value to consider some of the implications of their use.

The use of conformity as an ethical norm has certain important implications concerning the problem of social progress. If conformity is held to be desirable, then it is implicitly affirmed that the existing norm is desirable. In that event, progress must be considered either as automatic or else undesirable. That is to say, if conformity be held desirable, then progress is impossible unless it is considered inevitable. Mention was made before of the Spencerian ideal of perfect adjustment. Such an ideal was at least logically defensible in Spencer's philosophy, because he considered that progress is an inevitable consequence of the 'force' of evolution working through the 'law' of the survival of the fittest. Anyone who does not conceive of progress as inevitable or automatic, however, cannot consistently advocate uncritical conformity to social institutions.

In the second place, conformity as an ethical norm implies no discrepancy between what *is* and what *should be*. Anyone who uses conformity as an ethical norm by implication assumes that the social 'system' which is made up of all existing institutions is the best of all possible systems. To illustrate: if a psychiatrist or consultant judges an individual to be socially maladjusted—an ethical judgment denoting a condition which *should not* exist—it is implicitly assumed that the social *status quo*, to which the individual is maladjusted, is desirable; that things as they are, socially speaking, are as they should be. This is true whether the individuals concerned are living in Russia, in Germany, or in the United States. One who does not consider that the existing social situation is as it should be, and who nevertheless continues to

use social conformity uncritically as a norm in his treatment of adjustment problems, is not only inconsistent but immoral.

What may be said concerning the use of minimal tension as an ethical norm? This norm is quite explicit in the following excerpt from Socrates' last speech before the Athenian tribunal (4):

If a person were to select the night in which his sleep was undisturbed even by dreams, and were to compare with this the other days and nights of his life, and then were to tell us how many days and nights he had passed in the course of his life better and more pleasantly than this one, I think that any man, I will not say a private man, but even the great king himself will not find many such days or nights, when compared with the others.

Such an attitude is understandable and even, perhaps, quite sensible in a man who has just been sentenced to death. But it is not typical of Socrates throughout his life, and is an attitude which leads logically to suicide, unless one wonders with Hamlet whether death is a dreamless sleep. If a heightened awareness of the values in life, as exhibited by the great minds in history, is held to be desirable, and if, as Dewey maintains, we never think until we face a crisis of some sort, minimal tension as an ethical norm must be rejected. It is conceivable that an infant might be reared in an environment so controlled that conflict with it would be kept at a minimum, with a resulting minimum of tension. If intellectual and emotional growth, however, are considered to be largely functions of the necessity of solving problems, such an individual would remain forever immature.

This same criticism may be made of the ethical use of integration as an adjustment norm. Integration implies a minimum of conflict within the personality. A cursory glance at the biographies of the great literary, artistic, and religious personalities of history, however, is sufficient to make highly questionable the uncritical use of this norm. It is difficult, if not impossible, to separate the conflicts of Augustine, Poe, Tchaikowsky, or Tolstoi, to mention but a few, from their genius. Concerning this point Cattell (2, p. 198) says:

Though the psychologist may cure, and the teacher prevent, maladjustments in the child of average ability, there are certain forms of crooked personality in the gifted individual which they may straighten out only at the peril of society.

The difficulties with the ethical use of integration are not limited to its application to the person of outstanding ability. Consistency of response is, as Stagner (8, p. 139) indicates, the exception rather than the rule. The individual who part of the time is dominant, say in the office situation, and yet at home is submissive, illustrates the problem. Is the individual to be integrated as a dominant character or as a submissive character? If as either one or the other, on what grounds? Should we people the earth with dominant personalities or with submissive ones? This is not to say that the term integration is without value in descriptive use, but that its use as an ethical norm has certain definite limitations.

While the foregoing criticisms of certain ethical norms have by no means solved the problem of what norm should be used, their consideration has, it is hoped, brought light to the problem. The problem has a twofold nature, as we have seen. The norm to which we adjust the individual considered as an individual should be consistent with the social norm set up. If progress is to be made, both the individual and society must be changed. The tendency on the part of those engaged in the practical work of attempting to aid in the solution of adjustment problems to concern themselves entirely with the problem of the individual's adjustment to society, and thus to neglect the possibility that it may be society which should be adjusted, must be overcome. Social progress comes only through non-conformity. To borrow an example from the mathematical world, the theoretical work of Einstein at the time it was first set forth represented the opinion of only a very small fraction of one per cent of living mathematicians. Thirty-six years later it represents a majority opinion. Thus it is with all progress. It necessarily must start out as an opposition to the existing norm.

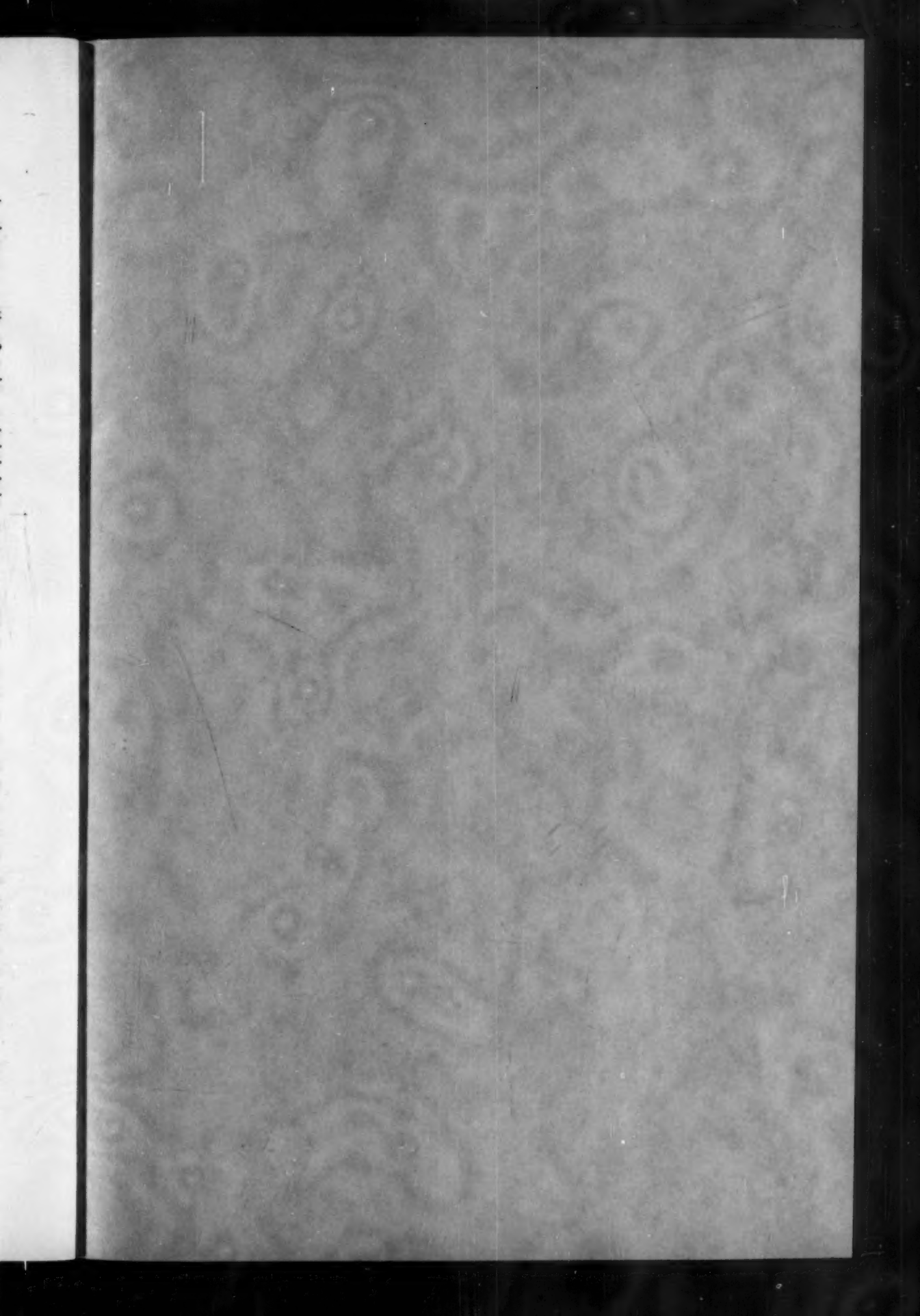
On the other hand, those zealous individuals who would reform society must overcome their tendency to overlook in-

dividual psychological problems, including their own. They must come to realize that security is often more a psychological or 'spiritual' problem than an economic one.

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